FOR·THE·INCREASE·AND·DIFFVSION·OF·KNOWLEDGE·AMONG·MEN·
PER ORBEM
SMITHSONIAN·INSTITVTION·
WASHINGTON·1846·

38TH CONGRESS, 1st Session. | HOUSE OF REPRESENTATIVES. | MIS. DOC. No. 83.

ANNUAL REPORT

OF

THE BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING THE

OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION FOR THE YEAR 1863.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1864.

In the House of Representatives, *June* 28, 1864.

Resolved, That five thousand extra copies of the Report of the Smithsonian Institution be printed—two thousand for the Institution. and three thousand for the use of the members of this House.

ANNUAL REPORT OF THE BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION UP TO JANUARY, 1864, AND THE PROCEEDINGS OF THE BOARD UP TO MARCH 16, 1864.

To the Senate and House of Representatives:

In obedience to the act of Congress of August 10, 1846, establishing the Smithsonian Institution, the undersigned, in behalf of the Regents, submit to Congress, as a report of the operations, expenditures, and condition of the Institution, the following documents:

1. The Annual Report of the Secretary, giving an account of the operations of the Institution, during the year 1863.
2. Report of the Executive Committee, giving a general statement of the proceeds and disposition of the Smithsonian fund, and also an account of the expenditures for the year 1863.
3. Proceedings of the Board of Regents up to March 16, 1864.
4. Appendix.

Respectfully submitted,

R. B. TANEY, *Chancellor.*
JOSEPH HENRY, *Secretary.*

LETTER

OF THE

SECRETARY OF THE SMITHSONIAN INSTITUTION,

COMMUNICATING

THE ANNUAL REPORT OF THE OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION FOR THE YEAR 1863.

JUNE 28, 1864.—Read, and ordered to be printed.

SMITHSONIAN INSTITUTION,
Washington, June 27, 1864.

SIR: In behalf of the Board of Regents, I have the honor to submit to the House of Representatives of the United States the annual report of the operations, expenditures, and condition of the Smithsonian Institution for the year 1863.

I have the honor to be, very respectfully, your obedient servant,

JOSEPH HENRY,
Secretary Smithsonian Institution.

Hon. S. COLFAX,
Speaker of the House of Representatives.

OFFICERS OF THE SMITHSONIAN INSTITUTION.

JANUARY, 1864.

ABRAHAM LINCOLN, *Ex officio* Presiding Officer of the Institution.
ROGER B. TANEY, Chancellor of the Institution.

JOSEPH HENRY, Secretary of the Institution.
SPENCER F. BAIRD, Assistant Secretary.
W. W. SEATON, Treasurer.
WILLIAM J. RHEES, Chief Clerk.

A. D. BACHE,
JOSEPH G. TOTTEN, } Executive Committee.
R. WALLACH,

REGENTS OF THE INSTITUTION.

H. HAMLIN, Vice-President of the United States.
ROGER B. TANEY, Chief Justice of the United States.
R. WALLACH, Mayor of the City of Washington.
W. P. FESSENDEN, member of the Senate of the United States, (Maine.)
L. TRUMBULL, member of the Senate of the United States, (Illinois.)
GARRETT DAVIS, member of the Senate of the United States, (Kentucky.)
S. S. COX, member of the House of Representatives, (Ohio.)
J. W PATTERSON, member of the House of Representatives, (New Hampshire.)
H. W. DAVIS, member of the House of Representatives, (Maryland.)
W. B. ASTOR, citizen of New York.
W. L. DAYTON, citizen of New Jersey.
T. D. WOOLSEY, citizen of Connecticut.
LOUIS AGASSIZ, citizen of Massachusetts.
ALEXANDER D. BACHE, citizen of Washington.
JOSEPH G. TOTTEN, citizen of Washington.

MEMBERS EX OFFICIO OF THE INSTITUTION.

ABRAHAM LINCOLN, President of the United States.
HANNIBAL HAMLIN, Vice-President of the United States.
W. H. SEWARD, Secretary of State.
S. P. CHASE, Secretary of the Treasury.
E. M. STANTON, Secretary of War.
G. WELLES, Secretary of the Navy.
M. BLAIR, Postmaster General.
E. BATES, Attorney General.
ROGER B. TANEY, Chief Justice of the United States.
D. P. HOLLOWAY, Commissioner of Patents.
RICHARD WALLACH, Mayor of the City of Washington.

HONORARY MEMBERS.

BENJAMIN SILLIMAN, of Connecticut.
J. P. USHER, Secretary of the Interior, (*ex officio*

PROGRAMME OF ORGANIZATION

OF THE

SMITHSONIAN INSTITUTION.

[PRESENTED IN THE FIRST ANNUAL REPORT OF THE SECRETARY, AND ADOPTED BY THE BOARD OF REGENTS, DECEMBER 13, 1847.]

INTRODUCTION.

General considerations which should serve as a guide in adopting a Plan of Organization.

1. WILL OF SMITHSON. The property is bequeathed to the United States of America, "to found at Washington, under the name of the SMITHSONIAN INSTITUTION, an establishment for the increase and diffusion of knowledge among men."

2. The bequest is for the benefit of mankind. The government of the United States is merely a trustee to carry out the design of the testator.

3. The Institution is not a national establishment, as is frequently supposed, but the establishment of an individual, and is to bear and perpetuate his name.

4. The objects of the Institution are, 1st, to increase, and, 2d, to diffuse knowledge among men.

5. These two objects should not be confounded with one another. The first is to enlarge the existing stock of knowledge by the addition of new truths; and the second, to disseminate knowledge, thus increased, among men.

6. The will makes no restriction in favor of any particular kind of knowledge; hence all branches are entitled to a share of attention.

7. Knowledge can be increased by different methods of facilitating and promoting the discovery of new truths; and can be most extensively diffused among men by means of the press.

8. To effect the greatest amount of good, the organization should be such as to enable the Institution to produce results, in the way of increasing and diffusing knowledge, which cannot be produced either at all or so efficiently by the existing institutions in our country.

9. The organization should also be such as can be adopted provisionally; can be easily reduced to practice, receive modifications, or be abandoned, in whole or in part, without a sacrifice of the funds.

10. In order to compensate, in some measure, for the loss of time occasioned by the delay of eight years in establishing the Institution,

a considerable portion of the interest which has accrued should be added to the principal.

11. In proportion to the wide field of knowledge to be cultivated, the funds are small. Economy should therefore be consulted in the construction of the building; and not only the first cost of the edifice should be considered, but also the continual expense of keeping it in repair, and of the support of the establishment necessarily connected with it. There should also be but few individuals permanently supported by the Institution.

12. The plan and dimensions of the building should be determined by the plan of organization, and not the converse.

13. It should be recollected that mankind in general are to be benefited by the bequest, and that, therefore, all unnecessary expenditure on local objects would be a perversion of the trust.

14. Besides the foregoing considerations deduced immediately from the will of Smithson, regard must be had to certain requirements of the act of Congress establishing the Institution. These are, a library, a museum, and a gallery of art, with a building on a liberal scale to contain them.

SECTION I

Plan of Organization of the Institution in accordance with the foregoing deductions from the will of Smithson.

To increase knowledge. It is proposed—

1. To stimulate men of talent to make original researches, by offering suitable rewards for memoirs containing new truths; and,

2. To appropriate annually a portion of the income for particular researches, under the direction of suitable persons.

To diffuse knowledge. It is proposed—

1. To publish a series of periodical reports on the progress of the different branches of knowledge; and,

2. To publish occasionally separate treatises on subjects of general interest.

DETAILS OF THE PLAN TO INCREASE KNOWLEDGE.

I.—*By stimulating researches.*

1. Facilities to be afforded for the production of original memoirs on all branches of knowledge.

2. The memoirs thus obtained to be published in a series of volumes, in a quarto form, and entitled Smithsonian Contributions to Knowledge.

3. No memoir on subjects of physical science to be accepted for publication which does not furnish a positive addition to human knowledge, resting on original research; and all unverified speculations to be rejected.

4. Each memoir presented to the Institution to be submitted for examination to a commission of persons of reputation for learning in

the branch to which the memoir pertains; and to be accepted for publication only in case the report of this commission be favorable.

5. The commission to be chosen by the officers of the Institution, and the name of the author, as far as practicable, concealed, unless a favorable decision be made.

6. The volumes of the memoirs to be exchanged for the transactions of literary and scientific societies, and copies to be given to all the colleges and principal libraries in this country. One part of the remaining copies may be offered for sale; and the other carefully preserved, to form complete sets of the work, to supply the demand from new institutions.

7. An abstract, or popular account, of the contents of these memoirs to be given to the public through the annual report of the Regents to Congress.

II.—*By appropriating a part of the income, annually, to special objects of research, under the direction of suitable persons.*

1. The objects, and the amount appropriated, to be recommended by counsellors of the Institution.

2. Appropriations in different years to different objects, so that, in course of time, each branch of knowledge may receive a share.

3. The results obtained from these appropriations to be published, with the memoirs before mentioned, in the volumes of the Smithsonian Contributions to Knowledge.

4. Examples of objects for which appropriations may be made.

(1.) System of extended meteorological observations for solving the problem of American storms.

(2.) Explorations in descriptive natural history, and geological, magnetical, and topographical surveys, to collect materials for the formation of a physical atlas of the United States.

(3.) Solution of experimental problems, such as a new determination of the weight of the earth, of the velocity of electricity, and of light; chemical analyses of soils and plants; collection and publication of scientific facts accumulated in the offices of government.

(4.) Institution of statistical inquiries with reference to physical, moral, and political subjects.

(5.) Historical researches and accurate surveys of places celebrated in American history.

(6.) Ethnological researches, particularly with reference to the different races of men in North America; also, explorations and accurate surveys of the mounds and other remains of the ancient people of our country.

DETAILS OF THE PLAN FOR DIFFUSING KNOWLEDGE.

I.—*By the publication of a series of reports, giving an account of the new discoveries in science, and of the changes made from year to year in all branches of knowledge not strictly professional.*

1. These reports will diffuse a kind of knowledge generally interesting, but which, at present, is inaccessible to the public. Some of

the reports may be published annually, others at longer intervals, as the income of the Institution or the changes in the branches of knowledge may indicate.

2. The reports are to be prepared by collaborators eminent in the different branches of knowledge.

3. Each collaborator to be furnished with the journals and publications, domestic and foreign, necessary to the compilation of his report; to be paid a certain sum for his labors, and to be named on the title-page of the report.

4. The reports to be published in separate parts, so that persons interested in a particular branch can procure the parts relating to it without purchasing the whole.

5. These reports may be presented to Congress for partial distribution, the remaining copies to be given to literary and scientific institutions, and sold to individuals for a moderate price.

The following are some of the subjects which may be embraced in the reports:

I. PHYSICAL CLASS.

1. Physics, including astronomy, natural philosophy, chemistry, and meteorology.
2. Natural history, including botany, zoology, geology, &c.
3. Agriculture.
4. Application of science to art.

II. MORAL AND POLITICAL CLASS.

5. Ethnology, including particular history, comparative philology, antiquities, &c.
6. Statistics and political economy.
7. Mental and moral philosophy.
8. A survey of the political events of the world, penal reform, &c.

III. LITERATURE AND THE FINE ARTS.

9. Modern literature.
10. The fine arts, and their application to the useful arts.
11. Bibliography.
12. Obituary notices of distinguished individuals.

II.—*By the publication of separate treatises on subjects of general interest.*

1. These treatises may occasionally consist of valuable memoirs translated from foreign languages, or of articles prepared under the direction of the Institution, or procured by offering premiums for the best exposition of a given subject.

2. The treatises should, in all cases, be submitted to a commission of competent judges previous to their publication.

3. As examples of these treatises, expositions may be obtained of the present state of the several branches of knowledge mentioned in the table of reports.

SECTION II.

Plan of organization, in accordance with the terms of the resolutions of the Board of Regents providing for the two modes of increasing and diffusing knowledge.

1. The act of Congress establishing the Institution contemplated the formation of a library and a museum ; and the Board of Regents, including these objects in the plan of organization, resolved to divide the income* into two equal parts.

2. One part to be appropriated to increase and diffuse knowledge by means of publications and researches, agreeably to the scheme before given. The other part to be appropriated to the formation of a library and a collection of objects of nature and of art.

3. These two plans are not incompatible one with another.

4. To carry out the plan before described, a library will be required, consisting, first, of a complete collection of the transactions and proceedings of all the learned societies in the world; second, of the more important current periodical publications, and other works necessary in preparing the periodical reports.

5. The Institution should make special collections, particularly of objects to illustrate and verify its own publications.

6. Also, a collection of instruments of research in all branches of experimental science.

7. With reference to the collection of books, other than those mentioned above, catalogues of all the different libraries in the United States should be procured, in order that the valuable books first purchased may be such as are not to be found in the United States.

8. Also, catalogues of memoirs, and of books and other materials, should be collected for rendering the Institution a centre of bibliographical knowledge, whence the student may be directed to any work which he may require.

9. It is believed that the collections in natural history will increase by donation as rapidly as the income of the Institution can make provision for their reception, and, therefore, it will seldom be necessary to purchase articles of this kind.

10. Attempts should be made to procure for the gallery of art casts of the most celebrated articles of ancient and modern sculpture.

11. The arts may be encouraged by providing a room, free of expense, for the exhibition of the objects of the Art-Union and other similar societies.

12. A small appropriation should annually be made for models of antiquities, such as those of the remains of ancient temples, &c.

13. For the present, or until the building is fully completed, besides the Secretary, no permanent assistant will be required, except one, to act as librarian.

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* The amount of the Smithsonian bequest received into the Treasury of the United States is	$515,169 00
Interest on the same to July 1, 1846, (devoted to the erection of the building)	242,129 00
Annual income from the bequest	30,910 14

14. The Secretary, by the law of Congress, is alone responsible to the Regents. He shall take charge of the building and property, keep a record of proceedings, discharge the duties of librarian and keeper of the museum, and may, with the consent of the Regents, employ assistants.

15. The Secretary and his assistants, during the session of Congress, will be required to illustrate new discoveries in science, and to exhibit new objects of art; distinguished individuals should also be invited to give lectures on subjects of general interest.

This programme, which was at first adopted provisionally, has become the settled policy of the Institution. The only material change is that expressed by the following resolutions, adopted January 15, 1855, viz:

Resolved, That the 7th resolution passed by the Board of Regents, on the 26th of January, 1847, requiring an equal division of the income between the active operations and the museum and library, when the buildings are completed, be, and it is hereby, repealed.

Resolved, That hereafter the annual appropriations shall be apportioned specifically among the different objects and operations of the Institution, in such manner as may, in the judgment of the Regents, be necessary and proper for each, according to its intrinsic importance, and a compliance in good faith with the law.

REPORT OF THE SECRETARY.

To the Board of Regents:

GENTLEMEN: I have the honor to present, at the commencement of another session of your honorable board, the annual report of the condition and transactions of the Smithsonian Institution during the year 1863.

The general operations of the Institution are so uniform from year to year that the several annual reports can differ but little from each other; the usual order will, therefore, be observed in this communication, with only such variations as the special incidents of the year may require.

It will be seen by the report of the Executive Committee that the finances of the Institution are in as favorable a condition as the state of public affairs would authorize us to expect. First. The whole amount of money originally derived from the bequest of Smithson is still in the treasury of the United States, bearing interest at six per cent., paid semi-annually, and yielding $30.910. Second. Seventy-five thousand dollars of an extra fund are in bonds of the State of Indiana, at five per cent. interest, also paid semi-annually, yielding $3,750. Third. Fifty-three thousand five hundred dollars of the same fund are in bonds of the State of Virginia, twelve thousand in those of Tennessee, and five hundred in those of Georgia, from which nothing has been derived since the commencement of the war. Fourth. A balance of upwards of $32,000 is now in the hands of the treasurer of the Institution.

The unsettled accounts at the close of the year do not exceed two thousand dollars.

From this statement it appears that the Institution, after erecting a building, accumulating a large library and an extensive museum, supplying the principal museums of the world with specimens of natural history, and publishing a series of volumes which have been distributed to all first-class libraries abroad, and still more extensively at home, has upwards of one hundred thousand dollars in addition to the money received from the original bequest. In addition to this, the stocks of Virginia and Tennessee are quoted at about half

their par value, and it may be a question whether they should not be disposed of and the money otherwise invested.

A part of the original bequest, amounting to £5,015, was left by Mr. Rush in England, as the principal of an annuity to be paid to the mother of the nephew of Smithson. The annuitant having died, a power of attorney was sent, in November, 1862, to Messrs. Fladgate, Clark & Finch to collect the money; but it has not yet been received. Although the whole legacy was awarded to Mr. Rush in behalf of the United States, after an amicable suit in chancery, various objections have been raised to allowing the small remainder to be sent to this country. These objections appear to be principally of a technical character, and are scarcely compatible with an equitable interpretation of the facts of the case. There should be no prejudice in England in regard to the construction placed upon the terms of the bequest and the policy which has been adopted, since one hundred and sixty-nine institutions in Great Britain and Ireland are recipients of the Smithsonian publications and specimens of natural history, and have enjoyed the advantages of its system of international exchange.

Although the financial affairs of the Institution are still in a favorable condition, its ability to produce results is materially diminished on account of the advanced prices of labor and materials, and especially the high rate of exchange under which its foreign operations are necessarily conducted. Still, all parts of the general system have been carried on with less abatement than might have been expected, as will be seen from the following account of the various operations:

Publications.—The publications of the Institution, as stated in previous reports, consist of three series: 1st, Contributions to Knowledge; 2d, Miscellaneous Collections; and, 3d, Annual Reports.

The Contributions include memoirs, embracing the records of original investigations and researches, resulting in such new truths as are considered interesting additions to knowledge. Twelve volumes in quarto of this series have been published, and the thirteenth is nearly ready for distribution.

The Miscellaneous Collections include works intended to facilitate the study of the various branches of natural history, to give instruction as to the method of observing phenomena, and to furnish a variety of other matter connected with the progress of science. Of this series four large octavo volumes have been issued, and two more are nearly completed.

The Annual Reports to Congress consist, each, of an octavo volume of 450 pages. They contain the report of the Secretary on the operations and condition of the Institution, the proceedings of the Regents, and an appendix, giving a synopsis of the lectures delivered at the Institution, extracts from correspondence, and articles of a character suited to meteorological observers, to teachers, and other persons especially interested in the promotion of knowledge.

The thirteenth volume of the Contributions has been completed, and is now in the hands of the binder. It contains the following original papers:

1. Tidal Observations in the Arctic Seas; by Elisha Kent Kane, M. D; made during the second Grinnell Expedition in Search of Sir John Franklin in 1853–55. Reduced and discussed by Charles A. Schott, assistant United States Coast Survey.

2. Meteorological Observations in the Arctic Seas; by Sir Leopold McClintock; made on board the Arctic searching yacht "Fox" in Baffin's Bay and Prince Regent's Inlet in 1857–'59. Reduced and discussed by Charles A. Schott, assistant United States Coast Survey.

3. Ancient Mining on the shores of Lake Superior. By Charles Whittlesey.

4. Discussion of the Magnetic and Meteorological Observations made at the Girard College Observatory, Philadelphia, in 1840–'45. Part II. Investigation of the Solar-Diurnal Variation of the Magnetic Declination and its Annual Inequality. By A. D. Bache, Superintendent Coast Survey.

5. Part III. Investigation of the Lunar Effects of the Magnetic Declination. By A. D. Bache.

6. Parts IV, V, VI. Horizontal Force. Investigation of the ten or eleven year period, and of the disturbances of the horizontal component of the magnetic force; investigation of the solar-diurnal variation and of the annual inequality of the horizontal force, and of the lunar effect on the same. By A. D. Bache.

7. Records and Results of a Magnetic Survey of Pennsylvania and parts of adjacent States in 1840, 1841, with some Additional Records and Results of 1834, 1835, 1843, and 1862, and a Map. By A. D. Bache.

8. Researches upon the Anatomy and Physiology of Respiration in the Chelonia. By S. Weir Mitchell, M. D., and George R. Morehouse, M. D.

Accounts have been given in previous reports of all the papers contained in this volume, excepting that on the Chelonia. This

paper, by S. Weir Mitchell, M. D., and George R. Morehouse, M. D., of Philadelphia, is a very complete study of the anatomy and physiology of the breathing organs in turtles. It seems that, although at one time, and by a single observer, the true mode of the breathing of these animals was partially understood, it had long been neglected, and modern physiologists have taught that turtles forced air into the lungs as do frogs. Drs. Mitchell and Morehouse have shown that turtles breathe like mammals, by drawing air into the lungs by the aid of muscles situated in the flanks and on the outside of the lungs. Their paper contains a detailed account of the anatomy of the breathing organs of turtles, and is illustrated with numerous wood-cuts. The most novel discovery described by the authors is that of a chiasm or crossing from side to side of a portion of the nerves which supply the muscles of the larynx. Except the well-known facts as to similar crossings within the skull, no previous author has described any similar extra-cranial arrangement of nerves. The physiological uses of the laryngeal chiasm has been fully studied by Drs. Mitchell and Morehouse; and more recently Professor Wyman, led by their discovery, has described similar nerve arrangements in serpents and in certain birds.

The authors express their indebtedness to the Smithsonian Institution for the aid with which they were furnished in obtaining the requisite specimens for experiments and for dissection.

The following papers have been accepted for publication, and will form parts of the fourteenth volume of Contributions:

1st. Three additional parts of the series of discussion of the magnetic observations at Girard College, by Professor A. D. Bache.

2d. The result of a series of microscopical studies of the medulla oblongata, or the upper portion of the spinal marrow, by Dr. John Dean.

3d. A memoir on the palæontology of the Upper Missouri, by F. B. Meek and F. V. Hayden.

4th. An account of the photographical observatory and various experiments in regard to this subject, by Dr. Henry Draper, of New York.

5th. A monograph of the "Laridæ" or gulls, by Dr. Elliott Coues.

All these memoirs, except the last, are in the hands of the printer, or in process of illustration by the engraver.

In several of the preceding reports an account has been given of a series of reductions of the magnetic observations made from 1840 to 1845, inclusive, at Girard College, Philadelphia, by Professor Bache.

The first two of the papers of this series related to what is called the eleven-year period of the variation of the needle, which corresponds with the recurrence and frequency of the spots on the sun. The third paper relates to the influence of the moon on the variation of the needle. The fourth refers to the change in the horizontal part of the force of the earth's magnetism coinciding with the eleven-year period of the spots on the sun. The fifth relates to the effect of the sun in producing daily and annual variations in the horizontal component of the magnetic force. The sixth relates to the lunar influence on the horizontal magnetic force.

A particular account has been given of the result of all these investigations, which tend fully to corroborate the conclusions arrived at from observations in other parts of the world, that both the sun and moon are magnetic bodies, and exert an influence upon the polarity of the earth; and also that the magnetism of the sun has variations in intensity which are in some way connected with the appearance of spots on its surface, giving rise to the variations in those perturbations of the needle which have been called magnetic storms, and which present a periodical recurrence at an interval of about eleven years.

The influence of the moon is much less marked than that of the sun, and appears to be more analogous to the temporary magnetism induced in soft iron.

Parts VII, VIII, and IX of this series, now in the press, are a continuation of the same subject. Part VII contains the discussion of the effect of a change of temperature on the readings of the vertical force instrument.

If a magnetic needle could be supported perfectly free in space, so as to assume the direction into which it would be brought by the magnetic action of the earth, it would arrange itself in the line of what is called the dip, or the inclination of the needle. At the magnetic equator of the earth such a needle would be parallel to the horizon, but, departing from this line either to the north or the south, the inclination would increase continually until we arrive at the magnetic pole, when it would be vertical. It is plain that the full magnetic force of the earth, in the line of the dip, may be resolved into two others, viz., a horizontal force, or that which draws the ordinary magnetic needle back to the meridian when it has been deflected from this position; and, second, the vertical force which tends to draw the end of the needle down into the line of the dip. The fre-

quency of vibrations of a magnetic bar suspended by an untwisted thread, so as to be horizontal, gives the horizontal component of the force of the earth, while the vibrations of a similar bar placed in the plane of the dip, and poised horizontally like a scale-beam on two knife-edges, gives the variations in the vertical force. These vibrations, however, will be affected not only by the changes in the magnetism of the earth, but by that in the bar itself; and as the latter is affected by the temperature of the place, a series of observations and discussions was necessary to ascertain the corrections due to this cause. For this purpose the room was artificially heated and cooled; but the value of the correction was finally deduced from an investigation of the whole series of regular observations compared with the changes of temperature indicated by the hourly register of the thermometer.

The corrections for temperature were afterwards applied to all the observations. The larger disturbances were then separated from the body of the series in the same manner as had been done with regard to the horizontal force, by which means the effect of the monthly and yearly disturbance of the sun is exhibited analytically and graphically. From the results it appears that the number and aggregate amount of disturbances were least in 1844; that in each year the greatest number of disturbances occurs in March and September, and the least number in June, or, in other words, the maximum about the equinoxes, and the minimum about the solstices.

In an appendix to this paper the connexion of the appearance of the aurora borealis with the disturbances of the direction and force of the earth's magnetism is discussed. From the result of this discussion it appears that there is a periodicity of about eleven years in the recurrence of the frequency of the aurora, as well as in that of the great disturbances of the needle, and that these are coincident with each other and with the appearance of the spots on the sun.

The eighth part of the series gives the discussion of the daily and yearly variations due to the action of the sun on the vertical component of the magnetic force. The mean variation of the force is determined for each hour during each month and for the whole year, and also for the summer and the winter separately. These are expressed analytically and graphically, and an examination of the curve shows a principal maximum about 1 p. m., and a principal minimum about 9 a. m. There is an indication of a secondary maximum about 2 a. m., and a secondary minimum about 4 a. m.

In summer the curve appears to have but one greatest and one least ordinate occurring about noon and midnight. In winter the double feature of the curve becomes quite conspicuous.

The vertical force appears greater in May, June, July, and August, and less in the remaining months, with a range of about a hundred and five hundred thousandth part of the whole force.

The ninth part gives the investigation of the influence of the moon upon the vertical force ; also upon the direction and intensity of the total force. The methods of investigation are the same in this as in the preceding parts. The daily effects of the moon exhibit a principal maximum a little before the planet passes the upper meridian, and a principal minimum about three hours after it passes the lower meridian. The average epoch of the tide of vertical force is about one and a half hour in advance, apparently, of the culmination of the moon. A secondary variation of this force, though noticed, is very feeble. The subject of the time of greatest lunar disturbance is yet very imperfectly developed, and more observations in regard to it are desirable.

A comparison is also given in this paper between the observations made at Toronto and Philadelphia, and their accordances or differences are stated. The effect of the moon upon the direction and intensity of the total force is obtained by a combination of the vertical and horizontal components. From this part of the investigation it appears that the dip is greatest at 8 and 20 hours, and least at 3 hours and 13½, the range being equal to 3.6 seconds ; and also that the maximum strength of the earth is greatest at half-past 12 and 11, and least at 7½ and 17 hours, the results, from the observations at Toronto and Philadelphia, being remarkably coincident.

The next paper of the foregoing list is that by Dr. Dean, which comprises the anatomy of the medulla oblongata, both human and comparative, from the lowest roots of the hypoglossal nerve, through the upper roots of the auditory, including the hypoglossal, nasal, glossopharyngeal, abducens, facial, and auditory nerves. The objects of the investigation were principally as follows :

1st. To illustrate the topography of the medulla oblongata by means of a series of photographs, which might completely map out all the principal changes in structure as they successively occur, connected with the development of the different nerves, with the details which accompany the development of their nuclei and accessory ganglia.

2d. The study of the more minute *histological* details, such as the course of the nerve roots, their entrance into their respective nuclei

and connexion with nerve cells, the connexion of the nuclei with each other by nerve fibres passing from the roots and from the nerve cells, the structure of the olivary bodies which possess a peculiar interest from their resemblance in convoluted structure to the cerebrum and cerebellum.

3d. An attempt to show, notwithstanding the apparent difference of structure between the spinal cord and medulla oblongata, a difference which appears very considerable at first sight, that the *plan* of structure of the two is identical, that the general arrangement of parts strictly corresponds, that the relation of the nerve roots to their nuclei or cell groups is the same, and moreover the connexion established between the different nuclei is carried out on the same plan. The illustrations for this work were taken by the author himself directly from the microscopic dissections by photography. For the general edition the photographic illustrations have been copied on stone with great care by L. H. Bradford. The steel plates were engraved by J. W. Watts. Besides these, a limited number of photographic prints from the original negatives have been prepared by Dr. Dean himself for private distribution, and from these negatives other copies may be obtained either on direct application to the author or through the medium of this Institution.

This paper, which is the result of over two years of constant study, was referred to Dr. W. A. Hammond, of the United States army, and Professor Jeffries Wyman for critical examination, and was recommended by them for publication as a valuable addition both to human and to comparative anatomy.

The third paper accepted for publication is on the Palæontology of the Upper Missouri, by F. B. Meek and F. V. Hayden.

This work contains figures and descriptions of all the known invertebrate fossil remains of the various geological formations of Idaho, Dakota, Nebraska, and portions of Kansas. About 370 species, nearly all of which are new, are fully described, and the descriptions are accompanied by remarks on the relations of each species to allied forms from other districts in this country and Europe, both living and fossil—its geological range, geographical distribution, &c. The illustrations consist of about one thousand carefully drawn figures, occupying forty-five quarto plates.

In addition to full descriptions of species, the work also contains extended accounts of all the genera to which these fossils belong, with the synonymy of each genus, and remarks on its affinities to other genera, both living and extinct; and assigns the probable period of

its introduction, the time when it appears to have attained its maximum development, and that at which it is supposed to have died out, if not represented in our existing seas. At the head of each generic description the etymology of the name and the type of the genus, when known, are given. Full descriptions of each of the families including these genera are likewise given ; and at the end of each family description the names of all the genera, whether living or extinct. The introduction contains detailed descriptions of the various formations in which these fossils existed, with remarks on their synchronism with other American and European deposits.

A considerable portion of the specimens described and figured were collected by Dr. F. V. Hayden in the several expeditions into the regions of the Upper Missouri and Yellowstone, sent by the government under the command of Lieutenant (now Major General) G. K. Warren, of the United States Topographical Engineers, to whose scientific zeal and liberal encouragement we are indebted for much of the material upon which the work is founded. But besides these, a large number were collected by Dr. Hayden himself previous to his connexion with the exploring expeditions of the government. The specific descriptions of the fossils described in this work are therefore to be regarded as appearing in the joint names of Meek and Hayden, while the descriptions of the genera, and families, and the discussion of their relations, geological range, geographical distribution, &c., are to be accredited to Mr. Meek alone.

The first sketch of this work was prepared as a part of the report to Congress of the explorations of the above-mentioned regions, but Mr. Meek has since devoted almost three years exclusively to extending and completing the investigations; and as it is probable that Congress will make no provision for its publication, it has been adopted by the Institution, at the earnest recommendation of several eminent naturalists, and will be published in successive parts. All the specimens described are in the collections of the Institution, and as soon as the work is completed the numerous duplicates will be distributed, as types of the species, to various scientific institutions at home and abroad.

Miscellaneous Collections.—Several series of articles forming parts of the Miscellaneous Collections, as stated in previous reports, have been undertaken, of which some have been completed, some are still in hand, and others have been printed during the past year.

The first of these series is that relating to the shells of North America, and will consist of the following works :

1. Check lists of North American shells, by P. P. Carpenter, &c.

2. Circular relative to collecting shells.

3. Elementary introduction to the study of conchology, by P. P. Carpenter.

4. List of the species of shells collected by the United States exploring expedition, by the same author.

5. Descriptive catalogue of the shells of the west coast of the United States, Mexico, and Central America, by the same author.

6. Descriptive catalogue of the air-breathing shells of North America, by W. G. Binney.

7. Descriptive catalogue of several genera of water-breathing fresh water univalves, by the same author.

8. Descriptive catalogue of the *Melaniadæ*, or the remainder of the water-breathing fresh water univalves, by George W. Tryon.

9. Descriptive catalogue of the *Corbiculadæ* or *Cycladidæ*, a group of bivalves principally inhabiting fresh water, by Temple Prime.

10. Descriptive catalogue of the *Unionidæ*, or fresh water mussels.

11. Descriptive catalogue of the shells of the eastern coast of the United States, by William Stimpson.

12. Bibliography of North American conchology, by W. G. Binney.

13. Check list catalogue of cretaceous and jurassic fossils of North America, by F. B. Meek.

The first and second articles of this list were published in 1860, and described in the report for that year. The third was published in 1861 as a part of the annual report for 1860. A new edition would have been printed before this time, as a part of the Miscellaneous Collections, had we not been disappointed by a delay in procuring the expected use of wood-cuts for the illustration of the work from the British Museum. We have just learned, however, that the Museum has liberally granted the use of these wood-cuts; that they are now being copied in stereotype in England; and consequently the work will be completed without further delay.

The fourth and fifth articles are still in the hands of Mr. Carpenter, who has reported progress, which leads us to expect that they will be ready for the press during the present year.

Of the 6th, 7th, and 8th, the first draughts of the manuscripts have been completed, and a preliminary sketch of the conclusions of the authors as regards the names of the species has been printed in the form of proof-sheets, and distributed to conchologists, with a view to elicit criticisms and suggestions prior to final publication.

Many important additions and corrections have been obtained in this way which will add much to the value of the works. The request has been made that these proof-sheets should not be considered as expressing the final views of the authors, but only intended to obtain the information above mentioned.

The ninth article of the series, by Mr. Prime, is well advanced in printing, and will be completed in 1864. In addition to the purely North American species, it will contain descriptions and wood-cut figures of those of Central and South America, as well as of the West Indies, thus embracing all the members of the family found in the New World.

The tenth and eleventh articles are still in process of preparation, and the engraving of the wood-cuts for their illustration has commenced.

The twelfth article—the first part of the Bibliography of North American conchology by Mr. Binney, mentioned in the last report as in press—has been completed and distributed. It forms a volume of 650 pages, and contains a list of the publications of American authors relative to conchology in general. As might reasonably be expected, some omissions have occurred of titles of papers overlooked or not met with, but copies have been sent to all the working conchologists of the country, with the request to furnish rectifications and additions to be inserted in an appendix to the second part. This second part, which is now in the press, is intended to include an account of the writings of foreign naturalists relative to American conchology, and will also contain, beside the additions and corrections of the first volume, copious indexes of authors and names of genera and species. About 250 pages are stereotyped, and the whole work, probably filling over 500 pages, will be finished during 1864.

The thirteenth article, check list by Mr. Meek, has been completed and put to press. It contains a list of all the species of cretaceous fossils described by authors up to the end of 1863, and will constitute an important aid in the labor of cataloguing and labelling collections, being prepared in the same style as that of the check-lists of North American shells, published by the Institution some years ago, which have been so much sought after by conchologists and amateurs.

Another series of works belonging to the miscellaneous collections is intended to facilitate the study of American insects. Of this series the several articles are as follows:

1. Instructions for collecting and preserving insects.

2. Catalogue of the described Diptera (flies, musquitoes, &c.) of North America, by Baron Osten Sacken.

3. Catalogue of the described Lepidoptera (butterflies, moths, &c.) of North America, by Dr. Jno. G. Morris.

4. Classification of the Coleoptera (beetles, &c.) of North America, by Dr. Jno. L. Le Conte.

5. Synopsis of the described Neuroptera (dragon-flies, &c.) of North America, with a list of the South American species, by H. Hagen.

6. Synopsis of the described Lepidoptera of North America, part I. Diurnal and Crepuscular Lepidoptera, by Dr. Jno. G. Morris.

7. List of the Coleoptera of North America, with descriptions of new species, by Dr. Jno. L. Le Conte.

8. Monograph of the Diptera of North America, by H. Loew, with additions, by Baron Osten Sacken.

9. Monographs of Homoptera and Hemiptera, (chinches, roaches, &c.,) of North America, by P. R. Uhler.

10. Descriptive Catalogue of the Hymenoptera, (bees, wasps, &c.,) of North America, by H. De Saussure.

These have all been described in previous reports.

Of No. 8, (monograph of Diptera,) the first part was published in 1862. During the past year the second part has been printed, and forms a volume of 339 pages. The manuscript of a third part is in an advanced state of preparation by Dr. Loew, and when received will, as in the case of the two preceding parts, be intrusted to Baron Osten Sacken for translation under his direction. We must again, in this connexion, express our obligations to Baron Osten Sacken for his valuable assistance in the preparation and publication of these works.

Of No. 9, monographs of Homoptera and Hemiptera of North America, by P. R. Uhler, the manuscript is nearly completed, and will soon be received from the author.

Of No. 10, the manuscript of the first part (Catalogue of Hymenoptera) was received from the author during the past summer, and placed in the hands of Mr. E. Norton, of New York, who kindly offered to translate it from the original French and superintend its publication. It is now in the press, and will soon be completed.

In addition to the publications relating to shells and insects, the following, belonging also to the Miscellaneous Collections, have been prepared for the Institution:

1. Check-list of Minerals, by Thomas Egleston.

2. Instructions relative to Ethnology and Philology, by George Gibbs.

3. Comparative Vocabulary, by George Gibbs.

4. Dictionary of the Chinook Jargon or Trade Language of Oregon, by George Gibbs.

5. Monograph of the Bats of North America, by Dr. H. Allen, United States army.

No. 1 of these works has been prepared to aid in arranging and cataloguing the Smithsonian collection of minerals and the distribution of duplicate specimens, but it will also be of value in facilitating the study of mineralogy by furnishing printed labels and check-lists for exchanges. It presents a list of all the described species of minerals, with their chemical symbols and systems of crystallization, indicating those which are peculiar to the United States, the whole arranged according to the method adopted by Professor Dana in the last edition of his Manual of Mineralogy. For important additions and corrections, this work is indebted to the principal mineralogists of this country, to whom the proofs were submitted, and especially to Professor Dana, Professor Brush, and Dr. Genth. This list is completed, and will shortly be ready for distribution.

No. 2 of these works was printed in the Smithsonian annual report for 1861, but a large demand having arisen for it, it has been reprinted with corrections and additions, and now includes instructions for philological observation, rules for recording sounds and vocabularies, &c. In the latter part of the work Mr. Gibbs has received important assistance from Professor W. D. Whitney, of Yale College.

It includes directions for the collection of various specimens, hints for special inquiry, &c. Among the former are the skulls of American Indians, which in some cases are difficult to obtain, on account of the jealousy with which the natives guard the remains of their dead. Numerous tribes, however, have become extinct, or have removed from their former abodes. The remains of victims of war are often left where they fall, and the bones of slaves and of the friendless are neglected. Relics of these can be obtained without offence to the living. It is, however, of essential importance that most positive information should be obtained as to the nation or tribe to which a particular skull belongs. This may frequently be learned from the history of the migrations of the tribe, or from the character of the ornaments and utensils found with it.

Among the specimens of art which are designated as desirable are dresses, ornaments, bows and arrows, lances, saddles with their furniture, models of lodges, cradles, mats, baskets, gambling implements, models of canoes, paddles, fish-hooks, carvings in wood and stone, tools, &c.

American antiquities are especially indicated as objects of interest. They include the tools found in the northern copper mines, articles

inclosed in mounds, images, pottery, also the contents of ancient shell beds found on the sea-coast and bays, often deeply covered with earth and overgrown with trees; human remains, or implements of human manufacture, bearing the marks of tools or of subjection to fire, found in caves, beneath deposits of stony material formed by droppings from the roof; similar articles in salt-licks, likewise in deposits of sand and gravel, or such as evidently belonging to the drift period. Among other desiderata mentioned are the names of tribes, geographical position, number of individuals, physical constitution, such as stature, proportion of limbs, facial angle, color of skin, hair, and eyes; inscriptions, dress, food, dwellings, arts, trades, religion, government, social life, ceremonies, mode of warfare, medicine, literature, method of dividing time, history, &c.

These directions also include a list of words most important to be used in forming the vocabulary of a language. The pamphlet consists of thirty-four pages, and is distributed gratuitously to all who are desirous of aiding investigations of this character.

No. 3 is a vocabulary of the principal words of which the equivalents are desired in the languages of the American Indians. It has been prepared with great care by Mr. Gibbs after the usual models, presenting in parallel columns the words selected in English, French, Spanish, and Latin, leaving a blank column to be filled by the required equivalents in the dialect of any given tribe. It forms a pamphlet of eighteen pages, including two hundred and eleven different words, and is printed on letter paper, for convenience in filling up the blanks.

No. 4, the Chinook Jargon, is a collection of phrases made up from various languages, Indian and civilized, and constitutes the sole medium of communication with the Indian tribes of the northwest. In 1853 the Smithsonian Institution published a brief dictionary of this language, from a French manuscript presented by Dr. B. R. Mitchell and edited by Professor W. W. Turner. The article was in great demand, and the edition was soon exhausted. Mr. Gibbs, having paid particular attention to the Jargon during his long residence in Washington Territory, kindly offered to prepare a new edition with corrections and additions. This offer was readily accepted, and the dictionary has been published during the past year.

The vocabulary of the Chinook contains words of two dialects, the Chinook proper and the Clatsop, and perhaps also of the Wakiakum. The nation or rather family to which the generic name Chinook has been applied, formerly inhabited both banks of the Columbia river

from its mouth to the Grand Dalles, a distance of about one hundred and seventy miles, and was, as is usual among the sedentary Indians of the west, broken up into numerous bands. Mr. Hale, in his Ethnography of the United States Exploring Expedition, has divided these into the Upper and Lower Chinook. The present vocabulary belongs to those nearest the mouth of the river, of which there were five principal bands. The language of the bands further up the river departs more and more widely from the Chinook proper; indeed, so much so that the lower Indians could not have understood the upper ones without an interpreter. This vocabulary is not as full as could be wished, and the only reason for publishing it in its present condition is that the Indians speaking the language are so nearly extinct that no better digest is likely to be made in future.

In regard to the 5th article of the above series, the Monograph of Bats of North America, it may be stated that the mammalia of this continent have been studied and described generally by Audubon, Bachman, and also by Professor Baird of this Institution. These authors, however, have not included in their descriptions the cheiroptera, or bats. To supply this deficiency, Dr. Allen, of Philadelphia, has given his attention for several years to the careful study of the specimens of this animal in the principal museums of this country, and has presented the result of his labors to the Institution in the form of the monograph above mentioned. In this a detailed description is given of each of the genera and species with wood-cut figures of the skulls, heads, ears, and tails of such species as require this mode of illustration. The wood-cuts of this paper have been completed and the manuscript is now in the hands of the printer.

I may mention that the Institution is indebted to Mr. Figaniere, Portugese minister, for a very graphic account of an immense assemblage of bats which had been colonized for years in the upper part of a mansion house which he had purchased in Maryland. This account will be republished in the appendix to this report, as well as in the paper of Dr. Allen just described.

Reports.—The annual reports to Congress are printed at the expense of the government as public documents, with the exception of the wood-cuts, the cost of which is paid by the Institution. Previous to 1853 the reports were principally confined to an exposition of the operations of the Institution, and were published in pamphlet form; but since that date an appendix has been added to each report, which, with the other matter, has increased the size to that of a volume of four hundred and fifty pages. These reports now form a series of ten

volumes, beginning with that of 1853, and in order that this series might contain a history of the Institution from the beginning, the will of Smithson, the enactments of Congress in regard to it. and the several reports of the Secretary, previous to 1853, were republished in the appendix to that volume.

The report for 1862 contains, in the appendix, a eulogy on the late Senator Pearce, by Professor Bache; a course of lectures on Polarized Light, by F. A. P. Barnard, late president of the University of Mississippi; a course of lectures on Ethnology, by Professor Daniel Wilson, of the University of Toronto; an introduction to a course of lectures on the Study of High Antiquity, by A. Morlot, of Switzerland, translated for the Institution by the author; an account of the Articles on Archæology, published by the Smithsonian Institution, copied from the "Natural History Review," of England; a history of the French Academy of Sciences; eulogies on Von Buch and Thenard, a continuation of the series of memoirs of distinguished members of the French Academy, translated by C. A. Alexander, esq.; a Memoir of Isidore St. Hilaire, by Quatrefages, translated by a lady; a prize Memoir on the Catalytic Force, by T. L. Phipson; on Atoms, by Sir John Herschel; Classification of Books, by J. P. Lesley; Account of Human Remains from Patagonia, and Prize Questions of Scientific Societies.

Of this report the usual number of 10,000 copies was printed, of which 4,000 copies were given to the Institution, to be distributed in accordance with the following rules:

1. To all the meteorological observers who send records of the weather to the Institution.
2. To the collaborators of the Institution.
3. To donors to the museum and library.
4. To colleges and other educational establishments.
5. To public libraries, and literary and scientific societies.
6. To teachers, or individuals who are engaged in special studies, and who make direct application for them.

Owing to the many changes which have taken place in the residence and occupation of the correspondents of the Institution since the commencement of the war, it has not been thought advisable to send the reports to all whose names are on the record of distribution, but in most cases to wait until direct application is made by letter or otherwise for a copy of the work. Whenever a report is sent to any address a separate announcement is made of the fact enclosing a blank receipt to be signed and returned to the Institution.

On account of the large amount of printing required by the government in consequence of the war, the public printing office has been taxed to its utmost power; documents not required for immediate use have been delayed, and among others the report of the Institution for 1862 is still not quite completed. It is expected, however, that it will be ready for distribution in the course of a few weeks. The number of copies of the report ordered to be printed by Congress has varied in different years, and consequently in the increasing demand some of the volumes have been entirely exhausted. It may be a matter of consideration whether a new edition of the report for 1856, and perhaps for other years, might not be reprinted. To prevent the future exhaustion of the supply of the reports, Congress authorized the stereotyping of the last volume and the printing at any time, from the plates, of the whole or any part of its contents.

In view of the great cost of paper and the space required for storage, it has been thought advisable to stereotype the Contributions and Miscellaneous Collections, and to strike off only as many copies of each article as are required for immediate distribution. By the adoption of this plan, the ability to supply, to any extent, copies of works published hereafter will always exist, while no more need be printed than are actually required.

Ethnology.—From the first, the Institution has given considerable attention to the various branches of ethnology. Besides the additions to Indian archælogy which are to be found in the several volumes of its Contributions to Knowledge, it has published several papers on languages. In the report for 1860, a list of original manuscripts was given relating to the languages of the northwest coast of America, which had been received through the assistance of Mr. Alexander S. Taylor, of Monterey, California.

Several of these were copied at the expense of the Institution, with the intention of securing their preservation and subsequent publication. It has also been stated that a number of these manuscripts had been presented to Mr. J. G. Shea, of New York, to be published in a series which he has established under the title of "Library of American Linguistics." By presenting these to Mr. Shea for publication and purchasing from him for distribution to learned societies a number of copies, encouragement has been given to a laudable enterprize, undertaken solely to promote a favorite branch of learning, and with but little comparative expense to the Smithsonian fund. I regret, however, to state that the diminution of the effective income of the Institution will prevent further appropriations at present for this purpose. The following is a list of the works of Mr. Shea's

series, of which the Institution has aided the publication by purchasing copies for distribution:

1. Grammar of the Mutsun language, spoken at the mission of San Juan Bautista, Alta California; by Father Felipe Arroyo de la Cuesta.

2. Vocabulary of the language of San Antonio mission, California, by Father Bonaventure Sitjar.

3. Grammar and dictionary of the Yakama language, by Rev. Mie. Cles. Pandosy.

4. Vocabulary or Phrase Book of the Mutsun language, of Alta California, by Rev. Father Felipe Arroyo de la Cuesta.

5. Grammar of the Pima or Névome, a language of Sonora, from a manuscript of the XVIII century.

The first of these, the Mutsun grammar, was described in the last report. The second, the vocabulary of the native inhabitants of the San Antonio, or Sextapay, mission; it was printed from a manuscript forwarded to the Institution by Alexander S. Taylor, of California. The mission of San Antonio de Padua was founded in 1771. in the Sierra of Santa Lucia, twenty-five leagues southwest of Monterey; the authors of this vocabulary being the first missionaries. The tribe is sometimes known as *Tatché*, or *Telamé*, though Mr. Taylor calls it *Sextapay*. It is gradually disappearing; not more than fifty Indians still remain, although it is said they were, at one time, so numerous that the dialects spoken by them amounted to twenty.

The third is the grammar and dictionary of the Yakamas, a people inhabiting the region of the Yakama river—a stream rising in the Cascade range of mountains, and emptying into the Columbia above the junction of the Snake river. The name signifies the "stony ground," in allusion to the rocky character of the country. The author of the grammar, Father Pandosy, was for many years a resident among these Indians, and became well acquainted with their language. In the destruction of the buildings of the mission by fire, during the Indian war in Washington Territory, the original of the grammar was lost, and the translation, published by Mr. Shea, which was made some time previously, alone remained. It is to be regretted that a more extended dictionary than the one now published was also destroyed at the same time.

The fourth article is a vocabulary of the same language, of which the grammar constitutes the first of this series, and is by the same author; the words are given in the Mutsun and Spanish languages.

The fifth, the grammar of the Pima, with a vocabulary in the same language and in Spanish, was obtained in Toledo, Spain, and trans-

lated by Buckingham Smith, esq. This manuscript was probably taken to Spain after the suppression of the order of the Jesuits in Mexico, in 1767. The Pima language was spoken by the tribes from the river Yaqui, in Sonora, northward to the Gila, and even beyond the Colorado, eastward beyond the mountains in the province of Taraumara, and westward to the sea of Cortez. The phrases given in these works will preserve the knowledge of what constituted the food of the inhabitants; their manner of living, their character, and native customs, &c. This may prove of historic interest hereafter, if the facts be nowhere else more circumstantially authenticated.

Meteorology.—From 1856 to 1861 an appropriation was made from the agricultural fund of the Patent Office for assistance to the Institution in collecting and reducing statistics relative to the climate of the United States. This was commenced while the Patent Office was under the direction of Judge Mason, but was suddenly discontinued under a change of administration. The propriety of an appropriation for this purpose, from the fund above mentioned, must be evident to every one who reflects on the intimate connexion between meteorology and agriculture. A knowledge of the peculiarities of the climate of a country is an essential requisite for the adoption of a system of scientific culture. The average temperature of the spring, autumn, and of the growing season; the ratio of the number of unfavorable to favorable years; the amount of rain, and moisture; the average time of the occurrence of late and early frosts, are all facts of importance in the economical adaptation of the crops to a given locality, in order to obtain the maximum of produce from a definite amount of labor.

The money received from the Patent Office was expended in assisting to defray the expense of the reductions of the observations, and as soon as the appropriation was stopped we were obliged to discontinue this part of the operations. The Institution, however, still continues to derive some benefit from its association with the Patent Office, in receiving through it, free of postage, the returned registers from the different observers.

Unfortunately, the postage law adopted at the last session of Congress prevents the correspondents on agriculture and meteorology from sending their reports by mail unless prepaid. This arrangement almost entirely stops the reception of these articles, for, since the service rendered is gratuitous, the observers cannot be expected to bear this additional burden. It is to be hoped that Congress will so modify the law as to remove this obstruction to a correspondence of great importance to the agricultural interests of the country.

Owing to this restriction, the number of meteorological registers received during the past year has been diminished, and the transmission of nearly all of them would have been discontinued had not the Commissioner of Agriculture, in view of their value to his department, decided to advance to some of the observers the necessary postage stamps to affix to their registers. He would willingly have sent stamps to all, but the tax would have been too heavy for the office; he therefore found it necessary to limit the number, and in doing so endeavored to make such a selection as would secure registers from districts distributed as uniformly as possible in all the States. Those observers, therefore, who have not been supplied with stamps should infer from this no disparagement of their observations, for among those who have been omitted from the list are some whose registers are highly prized for their regularity and accuracy.

Before it was known that this arrangement would be made by the Commissioner a circular was sent from this Institution to all the observers, mentioning the new feature in the postage law, and requesting them to continue their observations, and retain the records until the law should be modified, or some arrangement could be made by which the observers would not be subject to the burden of postage.*

Under the new organization of the Department of Agriculture a renewed interest has been manifested by the Commissioner in the collection of meteorological statistics, and he has expressed the desire to co-operate with this Institution in continuing and extending the system of records of the weather which it had established with so much labor and expense.

In order to obtain and diffuse a knowledge of facts of immediate importance to agriculturists, the Commissioner has commenced the publication of a monthly bulletin giving the state of the crops, the condition of the weather, and various other items of importance which are daily received from observers, and which would lose a considerable portion of their value were they suffered to remain unpublished until the end of the year. For this bulletin the Institution supplies the meteorological materials, consisting of the mean, maximum, and minimum temperature and amount of rain for each month in different States, and also, for the purpose of comparison, the mean temperature and amount of rain for a series of five years, grouped by States;

* This law has been changed since the above was written, and observers can send their meteorological registers, or other communications, to the "Commissioner of Agriculture," *without prepayment of postage.*

together with tables of important atmospheric changes, and notices of auroras, meteors, and other periodical phenomena. The publication has been received with much favor by agriculturists, and is regarded with great interest by the observers, who are thus furnished promptly with a general summary of the principal features of the meteorology of each month in all parts of the country, with which they can compare their own observations.

In view of the value of the information thus furnished by the Institution, it is hoped that the previous appropriation will be renewed, and that the reductions which have been discontinued for the last four years may be resumed.

The second volume of the Results of Meteorological Observations made for the Institution, from 1854 to 1859, and reduced by Professor Coffin, is still in the press, its completion being delayed by the great pressure, upon the public printing office, of government work relative to the war.

We are indebted to the courtesy of Captain (now General) George G. Meade, of the topographical engineers, superintendent of the survey of the north and northwestern lakes, and of his successor in office, Lieutenant Colonel J. D. Graham, for a continuation of the favor formerly extended to the Institution in furnishing us with copies of the meteorological observations made at the different stations established for the survey. These records are very valuable, being made with full sets of instruments and at important places. They embrace observations made three times a day, at the same hours with the Smithsonian system, 7 a. m. and 2 and 9 p. m., and at ten stations, extending from Superior City in the State of Wisconsin, at the western extremity of Lake Superior, to Sackett's Harbor in New York, on the east end of Lake Ontario.

The Bureau of Medicine and Surgery of the Navy Department also continues to furnish us with the meteorological records kept at the naval hospitals at Chelsea, New York, and Philadelphia.

For several years previous to the commencement of the war a large map was exhibited in the Smithsonian Institution on which was daily represented the direction of the wind and face of the sky over the greater portion of the United States; and in previous reports we have frequently called attention to the fact that a properly organized system for giving daily or half daily changes of the weather in distant parts of the United States would be of great practical importance to the shipping interests of the country; we have also stated the fact that we are much more favorably situated for predicting the coming

weather than the meteorologists of Europe. The storms in our latitude generally move from west to east, and, since our seaboard is on the eastern side of a great continent, we can have information of the approaching storm while it is still hundreds of miles to the west of us. Not so with the meteorologists of Europe, since they reside on the western side of a continent, and can have no telegraphic dispatches from the ocean. The proposition, however, to furnish constant information of this kind could not be carried out by the limited means of the Smithsonian Institution, and, indeed, can only be rendered properly and fully serviceable under the direction and at the expense of the government.

New and interesting features have been introduced into the daily meteorological bulletin published by the Imperial Observatory at Paris. As mentioned in the last report, these bulletins are lithographed each day from records of the barometer, thermometer, wind, and face of the sky, compiled from telegraphic reports transmitted to the observatory from various parts of Europe. In addition to these, they now contain daily a small outline chart of Europe upon which are drawn diagrams showing the barometric curve of the day through the various stations, together with the temperature and direction and force of the wind. For the use of vessels about to leave port, a statement is also given of what will probably be the direction of the wind the next day. Chambers of commerce and intelligent seamen have acknowledged in strong language the benefit of these daily bulletins, thus adding to the ever-accumulating testimony in favor not only of the speculative interest but also practical benefits of meteorology. At Bordeaux, Havre, and other important ports, as soon as the bulletins are received, the telegraphic announcement of the weather and the probable direction of wind for the following day are posted in public places and furnished to the principal newspapers for publication. The bulletin also contains extracts from the correspondents of the observatory on astronomical and other subjects as well as meteorology. With the number for December 20, a supplement was issued with a diagram exhibiting the indications of the self-registering instruments at the Royal Observatory, Greenwich, during the great storm on the English coast in the first three days of December, 1863.

Laboratory.—The principal work which has been done in the laboratory during the past year is an extended series of experiments on the properties of different kinds of oil intended for light-house purposes. For a number of years past the price of sperm oil has been constantly increasing, and from a dollar per gallon it had ad-

vanced last year to two dollars and forty-three cents. It became, therefore, an important matter to the Light-house Board to determine whether some other burning material could not be introduced in the place of so expensive an article. The investigation of this subject was given in charge to myself, as the chairman of the Committee on Experiments. The result of the investigations not only revealed a number of new phenomena of interest to science, but also established the important practical fact of the superiority of winter strained lard oil over standard sperm oil in the intensity of the light, the steadiness and persistence of the flame, and the less care required in attendance. This fact must have an important bearing on the cost of lighting the extended coast of the United States, as well as upon the commercial value of one of the staple products of the western part of our Union. The price of lard oil is, at present, considerably less than one-half of that of sperm, and while the supply of sperm oil has remained stationary, or even diminished with an increasing demand, the sources of lard oil in the country are abundant, and the quantity which can be produced will be sufficient to meet almost an unlimited consumption.

Another series of experiments was made for determining the proper arrangements of reflectors and lenses for illuminating distant objects either by the electric or the calcium light. These experiments were instituted at the suggestion of the Navy Department, but as no appropriation was made for their being carried into practice, they were discontinued, and the knowledge obtained remains unapplied.

Collections of specimens of natural history, &c.—In several of the preceding reports a distinction has been drawn between the collection of specimens of natural history made through the agency of this Institution, and what is called the Smithsonian museum. The object of making large collections of duplicate specimens is, first, to advance science by furnishing to original investigators new materials for critical study; and second, to assist in diffusing knowledge, by providing colleges, academies, and other educational establishments, with labelled specimens to illustrate the various productions of nature, while the principal end to be attained by the public museum of the Institution is the gratification and instruction of the inhabitants and visitors of the city of Washington.

The collecting and distributing of a large number of specimens, for the purpose stated, is an important means of increasing and diffusing knowledge, and, as such, is in strict accordance with the will of the founder of the Institution. It has, therefore, from the first received

much attention, and has been attended with a commensurate amount of beneficial results. Among the collections received during the past year have been specimens of great interest, either the results of explorations, undertaken by the Institution, or of exchanges with individuals or local societies. The materials thus collected belong principally to two classes, namely, to specimens of new or rare forms intended to advance natural history and duplicates of such as are to be labelled and distributed for the purposes of education. Among the former are the collections of Mr. Kennicott, whose explorations have been mentioned in previous reports. They are of a very valuable character, illustrating the natural history and ethnology of the northwestern portions of the continent of North America. The specimens received in 1863, from this exploration, filled forty boxes and packages, weighing, in the aggregate, 3,000 pounds. They embrace in the line of natural history thousands of skins of mammals and birds, eggs, nests, skeletons, fishes, insects, fossils, plants, &c. In the line of ethnology are skulls, dresses, weapons, implements, utensils, instruments of the chase, in short, all the requisite material to illustrate the peculiarities of the Esquimaux and different tribes of Indians inhabiting the northwest regions.

In addition to the collections obtained from the British possessions in North America, by Mr. Kennicott, specimens have been received from other points and other parties. Among these are a series of birds and eggs from Labrador, gathered by Mr. Henry Connolly, and a large amount of new material from Mexico, collected by John Xantus, under the auspices and at the expense of the Institution, consisting of birds, fishes, reptiles, shells, &c. Another series from the same country has been presented by Dr. Sartorius, who has, for a number of years, been one of the meteorological observers of the Institution. Interesting collections have been received, also, from Dr. A. Van Frantzius, of Costa Rica; from Mr. Osbert Salvin, of Guatemala; from Captain J. M. Dow, of Panama; specimens from Cuba have been presented by Mr. C. Wright and Prof. Poey; from Trinidad, by Mr. Galody, United States consul; from Jamaica, by Mr. W. T. March; from Ecuador, by the Hon. C. T. Buckalew, now of the United States Senate. A valuable contribution of birds and mammals has also been received from Prof. Sumichrast, of Orizaba. These collections are all intended to illustrate the natural history of the American continents, to the investigation of whose extended regions the Institution has especially directed its labors.

In order to facilitate the preparation of a work on the birds of America, by Prof. Baird, a circular from the Institution was dis-

tributed through the State Department to the consular and diplomatic agents of the United States in Central and South America, asking aid in completing the collection of birds, and we doubt not that much new and valuable material will thus be obtained.

The following are the rules which have been adopted in regard to the disposition and use of the collections:

First. To advance original science, the duplicate type specimens are distributed as widely as possible to scientific institutions in this and other countries, to be used in identifying the species and genera which have been described.

Second. For the purposes of education, duplicate sets of specimens, properly labelled, are presented to colleges and other institutions of learning in this country.

Third. These donations are made on condition that due credit is to be given the Institution in the labelling of the specimens, and in all accounts which may be published of them.

Fourth. Specimens are presented to foreign institutions, on condition that if type specimens are wanted for comparison or other use in this country they will be furnished when required.

Fifth. In return for specimens which may be presented to colleges and other institutions, collections from localities in their vicinity shall be furnished when wanted.

In the disposition of the undescribed specimens of the collection, the following considerations have been observed as governing principles:

First. The original specimens are not to be intrusted for description to inexperienced persons, but to those only who have given evidence of ability properly to perform the work.

Second. Preference is to be given to those who have been engaged in the laborious and difficult enterprise of making complete monographs.

Third. The investigator may be allowed, in certain cases, to take the specimens to his place of residence, and to retain them for study a reasonable time.

Fourth. The use of the specimens is only to be allowed on condition that a series of types for the Smithsonian museum will be selected and properly labelled, and the whole returned in good condition.

Fifth. In any publications which may be made of results derived from an investigation of the materials from the Smithsonian collection, full credit must be accorded to the Institution for the facilities which have been afforded.

During the past year the assorting and labelling of the specimens have been continued, as well as the distribution of duplicates.

The whole number of entries on the record book of the Smithsonian collection, at the end of the year 1861, was 66,075 ; at the end of 1862, 74,764, and at the end of 1863, 86,847 ; but each entry indicates a lot consisting of a number of specimens. The whole number of duplicate specimens distributed to different institutions in this country and abroad, up to the end of the year 1863, has been 94,713. As these specimens are distributed on the express condition that full credit is to be given to the Institution on the labels, and in all publications which may relate to them, the name of Smithson, even through this distribution alone, would become familiarly known in every part of the civilized world.

It has been, from the first, one of the prominent objects of the Institution to collect the most ample materials for illustrating the entire natural history of North America ; to determine the different species of plants and of animals ; to ascertain the distribution of the former, and the migrations of the latter. This object it has endeavored to accomplish through the agency of the different surveying expeditions of government ; through explorations instituted at its own expense, and by enlisting the co-operation of individuals interested in science, and of local scientific societies. In all its efforts in this line it has been heartily supported, and it is believed that its labors have been productive of valuable results. The collections thus made have been intrusted to competent investigators for examination and description, and the results published in the different Smithsonian series, in transactions of societies, and in various government reports. For a list of what has already been prepared and published, either by the Institution or under its direction, I would refer to a report on this subject in preparation by Professor Baird.

Museum.—The additions to the museum, in the line of natural history, are principally confined to the type specimens which have been collected and described at the expense of the general government, or under the immediate auspices of the Institution. Even thus restricted, the specimens increase in number more rapidly than the portion of the Smithsonian fund which can be devoted to their support will authorize. Few persons have an idea of the labor, constant care, and expense which attends the proper preservation of a series of objects of natural history ; but those who have had the necessary experience know that large miscellaneous collections can only be properly supported by governments, and, in the establishment of provincial socie-

ties, the rule has been strongly recommended of attempting to preserve nothing except what is strictly local. "It is the experience of societies," says Dr. Jardine,* the celebrated Scotch naturalist, "that general collections are encumbrances, and in most instances get destroyed for want of care, or they are dispersed. Within these few years the really fine and valuable collection of the Zoological Society of London, chiefly presented by the late N. A. Vigors, a first-rate scholar and naturalist, and containing many unique things from our scientific exploratory voyages, has been sold. That of the Entomological Society has also been sold, and the greater part of that belonging to the Linnæan Society was sold during the last month, because there was not sufficient space to keep what had been presented to them. The collection of the Royal Society of Edinburgh is now undergoing the same process."

During the past year the work of labelling the specimens in the museum, so that the common, as well as the scientific name of each article may be distinctly exhibited, has been continued.

Explorations.—The only explorations during the past year, under the auspices and at the expense of the Institution, are, 1st, the continuation of that of Mr. Xantus on the western coast of Mexico ; and, 2d, that by Mr. Meek in New Jersey and the lower part of Virginia. The explorations of Mr. Xantus extended several hundred miles along the western coast of Mexico in a region little known, and very abundant in interesting objects.

The exploration of Mr. Meek related to the collection of complete series of shells to illustrate the tertiary formation of the seaboard of New Jersey and Virginia. Several series of shells were obtained, which are in the process of being accurately labelled, and are intended for distribution to some of the principal colleges of the country.

Exchanges.—The important aid rendered to science and literature by the system of international exchange which has for many years been actively carried on by the Institution, is still everywhere highly appreciated. Our operations in this line are becoming more and more extensive, requiring an additional amount of time, labor, and attention, as well as largely increasing in expense. The great liberality of many of the transportation companies alone enables us to carry on the system in its present extent, and we again tender our acknowledgments, especially to the following parties, who have

* Address of Sir W. Jardine, president of the Dumfriesshire and Galloway Natural History and Antiquarian Society, December, 1863

assisted us in this respect: The North German Lloyd, between Bremen and New York; the Hamburg and New York steamship line; the Cunard line; the Panama Railroad Company; the Pacific Mail Steamship Company; Adams's Express Company, and the Hudson's Bay Company.

During the past year it was deemed advisable to establish a new agency of exchanges for Holland and Belgium, and Mr. Fred. Muller, bookseller, at Amsterdam, who was appointed the agent, has entered upon the discharge of his duties with zeal and efficiency. The numbers of the transactions of the societies in the countries referred to necessary to complete the sets in the Smithsonian library, as well as much other valuable scientific and literary material, have been procured by him. The other foreign agents of the Institution are still Dr. Felix Flugel, Leipsic, Mr. Wesley, London, and Gustave Bossange, Paris.

From the tabular statement given by Professor Baird, it appears that during the year 1863 there have been sent to foreign countries 1,426 packages, each containing a number of articles, enclosed in 61 boxes, measuring 447 cubic feet, and weighing 10,286 pounds. The number of packages received in return for societies and individuals in this country was 1,522, included in which, for the Smithsonian Institution, were 4,589 books and pamphlets, besides specimens of natural history.

Library.—The policy in regard to the library as has frequently been previously stated, is to form a collection as perfect as possible of all the tranactions and proceedings of the learned societies of the world. The success of the Institution in this enterprise has been fully commensurate with the expectations entertained, and the collection of works of this class, if the accumulation continues under the same favorable conditions, will soon rival any other of a like kind in the world. The liberal distribution which the Institution has made of its own publications and those of government has produced a rich return in series of transactions which, although existing as duplicates in some of the older libraries of Europe, can scarcely be obtained by purchase.

It was mentioned in the last report that the number of transactions and proceedings of learned societies contained in the library of the Institution had increased so much that a new edition of the catalogue previously published had become necessary. This work has since been put to press, and will be printed as rapidly as the care necessary to insure acccuracy will permit. Copies of this catalogue

will be distributed to all the principal libraries of the country, and with the liberal policy which has been adopted in regard to the books of the Smithsonian collection, will serve to render the library more generally useful.

By exchanges there have been received 719 octavos, 167 quartos, and 24 folios; of parts of volumes and pamphlets, in octavo, 2,119; in quarto, 779; in folio, 581; maps and charts 200; total, 4,589. In addition to these about 400 volumes were purchased.

Among the valuable works received during the year, are the following:

55 volumes from the Royal Library of Stockholm.

Comptes-Rendus, 1859, 1860, 1861, with atlas, from the Commission Imperiale Archælogique, St. Petersburg.

12 volumes and 18 parts of volumes from the Koninlijk Institut des Ingenieurs, d'Gravenhage.

52 volumes and 94 pamphlets from the Nederlandsch Maatschappig ter Bevordering van Nijverheid, Haarlem.

10 volumes of its own publications from the Société pour la recherche et la conservation des Monuments Historiques du Grand Duché de Luxembourg, Luxembourg.

24 volumes and 12 parts from the Kaiserliche Akademie der Wissenschaften, Vienna.

9 volumes and 29 charts from the Etablissement Géographique de Bruxelles.

21 volumes of Proceedings from the Société d'Agriculture, Commerce, Science et Arts du Dept. de la Marne.

24 volumes of Proceedings and Transactions from the Institution of Civil Engineers, London.

36 volumes and 114 charts from the Board of Admiralty, London.

Large donations from the Royal University of Norway.

Braddam's Memoirs of the Royal Society of London, vol. I—X, 1745, from Mrs. Mary A. Malthie, Syracuse, New York.

26 volumes from the Regents of the University in behalf of the State of New York.

Lectures.—The usual course of lectures has been commenced for the present season, and will embrace the following:

Five lectures, by Rev. John Lord, of New York, on the "Fall of the Roman Empire." *Subjects.*—I. The grandeur and glory of the Ancient Civilization—The external splendor of the Roman Empire in its latter days. II. The internal hollowness and defects

of the old Roman civilization—The shame and miseries of society—The vices of self-interest, and preparation for violence and inevitable ruin. III. The fall of the empire, and the desolations produced by the barbarians—The destruction of the old fabric of society. IV. The reasons why the old conservative influences of paganism did not arrest the ruin—The failure of art, literature, and science, and the mechanism of governments. V. The reasons why Christianity did not save the Empire, and the ideas which the church incorporated with subsequent civilizations—The foundation of the new Teutonic structure.

Three lectures, by Professor Louis Agassiz, of Cambridge, Massachusetts, on the "Glacial period."

One lecture, by Professor J. L. Campbell, of Wabash College, on "Galileo."

Seven lectures, by Dr. Reinhold Solger, on "The Races of Men."

Six lectures, by Professor W. D. Whitney, of Yale College, on "Philology." I. History and objects of linguistic science—Plan of these lectures—Why and how do we speak English—How language is preserved and perpetuated—Its constant change—The study of language an historical science. II. Illustration of the processes of growth and change in language—Formation of words by combination of old materials—Mutilation and corruption of existing forms—Change and development of meaning—Rate of progress of these changes. III. Statement and illustration of the influences causing the growth of dialects, and those checking and counteracting this growth—Our language a Germanic dialect, with partly French vocabulary—Other languages with which it is related—Branches of the Indo-European family of languages, and proof that they are of common descent—Place, period, and grade of civilization of the original tribe. IV. Historical and linguistic importance of the Indo-European race and language—History of the language—Its development from monosyllabic roots. V. Survey of the other great families of language, Semitic, Scythian, Chinese, Polynesian, Egyptian, African, and American—Isolated languages not included in these families. VI. Comparative value of linguistic and physical evidence of race, and their relative bearing on the science of ethnology—Relation of the study of language to the question of the unity of the human race—origin of language—Its character and value to the human race.*

The number of applications for the use of the lecture-room has

* A synopsis of this course of lectures has been furnished by the author for insertion in the appendix to this report.

been much less since the adoption of the rule restricting its use to the purposes of the Institution exclusively has become more generally known. This rule, which has been widely approved of by the enlightened public, has precluded a large amount of unprofitable correspondence and enabled the Institution to avoid an embarrassing and inauspicious connexion with sensational expositions of the exciting subjects of the day.

From the preceding account of the present condition of the Institution, and of its operations during the past year, as well as from the examination of the collections and publications, it is hoped that, notwithstanding the unfavorable condition of the country for scientific research, and the diminished means at our command, it will appear that the line of policy and of action originally adopted has been pursued with unabated ardor and with corresponding success.

Respectfully submitted.

JOSEPH HENRY, *Secretary.*

WASHINGTON, 1864.

APPENDIX TO THE REPORT OF THE SECRETARY.

SMITHSONIAN INSTITUTION,
Washington, December 31, 1863.

SIR: I have the honor to present herewith a report, for 1863, of the operations intrusted to my charge, consisting especially of those relating to the printing, the exchanges, and the collections of natural history.

Very respectfully, your obedient servant,

SPENCER F. BAIRD,
Assistant Secretary Smithsonian Institution.

Prof. JOSEPH HENRY, LL.D.,
Secretary Smithsonian Institution.

PRINTING.

An accompanying table will show the works printed during the year, and also those now in press. The total number of pages belonging to works finished within the year is:

Of quarto papers	350 pages, 3 plates.
Octavo Miscellaneous Collections	1,313 pages.

Of works still in press there have been printed:

Of quarto works, about	108 pages.
Octavo	443 pages.

Making a total of 458 quarto pages, and 1,756 octavo, exclusive of the annual report to Congress, nearly finished, and to fill 450 pages.

EXCHANGES AND TRANSPORTATION.

The system of exchanges has been in a highly successful condition during 1863, both the receipts and transmissions being fully equal to the average of any previous year. The attendant expenses of this branch of operations are, however, great and increasing, and would long since have become almost prohibitory but for the liberality exhibited by various transportation companies in carrying the boxes and parcels of the Institution free of any charges for freight. It is not too much to say that thousands of dollars are thus presented by the companies as a recognition on their part of the great importance, domestic as well as international, of these operations of the Institution. Among the parties deserving of especial mention in this connexion are the proprietors of the Cunard steamers between New York and Liverpool, and New York and Havana; the North German Lloyd, between New York and Bremen; the Hamburg American Packet Company, between New York and Hamburg; the Panama Railroad Company; the Pacific Mail Steamship Company; the Hudson's Bay Company; the Adams Express Company, &c.

The Institution is under especial obligations, for important services rendered in this connexion, to the Hon. Hiram Barney, collector of the port of New York, and to his assistant, Mr. George Hillier; to Mr. A. B. Forbes and Mr. Hubbard, of the Pacific Mail Steamship Company, in San Francisco, as well as to the regular agents of the Institution.

During the year, 1863, a new literary agency of the Institution was established for Holland and Belgium. Mr. Frederick Müller, bookseller, of Amsterdam, was placed in charge, and he has already rendered much service. The other foreign agents of the Institution—Dr. Felix Flügel, of Leipsic; Gustave Bossange & Company, of Paris; and Mr. William Wesley, of London—continue to discharge their duties with efficiency, and to the full satisfaction of the Institution.

The number of institutions and individuals, at home and abroad, making use of the facilities of scientific exchanges offered by the Smithsonian Institution is continually on the increase, and it is believed that any interruption or suspension of this part of the programme of operations would be considered as a serious calamity.

In 1862 the Institution distributed four volumes of Miscellaneous Collections, one volume of Annual Reports, and one thick quarto volume of Meteorological Records and Reductions. In 1863, owing to various circumstances, the Annual Report for 1861 was the only volume distributed, although many copies of separate papers were sent abroad. For this reason the bulk of sending, in 1863, was less than that of previous years, but it is expected that the difference will be fully made up in 1864.

The following tables exhibit the details of the operations in the line of exchange during 1863:

A.

Receipt of books, &c., by exchange in 1863.

Volumes:		
Octavo	719	
Quarto	167	
Folio	24	
		910
Parts of volumes and pamphlets:		
Octavo	2, 119	
Quarto	779	
Folio	581	
		3, 479
Maps and charts		200
Total		4, 589
Receipts in 1861		2, 886
Receipts in 1862		5, 035

B.

Table showing the statistics of the exchanges of the Smithsonian Institution in 1863.

Agent and country.	Number of addresses.	Number of packages.	Number of boxes.	Bulk of boxes in cubic feet.	Weight of boxes in pounds.
DR. FELIX FLÜGEL, *Leipsic*—					
Scandinavia	1	1			
Sweden	13	31			
Norway	4	14			
Denmark	13	31			
Russia	42	76			
Germany	265	455			
Switzerland	30	58			
Belgium	10	24			
Total	378	690	26	202	3,500
FREDERICK MÜLLER, *Amsterdam*—					
Holland	40	81			
Total	40	81	4	31	1,006
GUSTAVE BOSSANGE & CO., *Paris*—					
France	107	180			
Italy	58	95			
Spain	7	16			
Portugal	4	8			
Total	176	299	13	88	2,800
W. WESLEY, *London*—					
Great Britain and Ireland	169	316	14	106	2,580
Rest of the world	20	40	4	20	400
Grand total	783	1,426	61	447	10,286

C.

Addressed packages received by the Smithsonian Institution from parties in America for foreign distribution in 1863.

	Number of packages.
Albany, N. Y.—	
Prof. James Hall	9
Boston, Mass.—	
American Academy of Arts and Sciences	116
Boston Society of Natural History	205
C. J. Sprague	1
Cambridge, Mass.—	
Harvard College	31
Museum of Comparative Zoology	342
Alexander Agassiz	34
Prof. Asa Gray	20
Jules Marcou	5

Cleveland, Ohio—	
Dr. J. S. Newberry	60
Columbus, Ohio—	
Ohio State Board of Agriculture	102
Detroit, Mich.—	
Lieut. Col. J. D. Graham, U. S. A	56
Janesville, Wis.—	
Institution for the Blind	73
Montreal, Can.—	
Prof. J. W. Dawson	7
New Haven, Conn.—	
American Journal of Science	18
American Oriental Society	8
Prof. J. D. Dana	15
New York, N. Y.—	
Mercantile Library	10
New York Lyceum of Natural History	101
Philadelphia, Pa.—	
Academy of Natural Sciences	165
Entomological Society of Philadelphia	9
Pharmaceutical Association	100
George W. Tryon	57
Santa Barbara, Cal.—	
A. S. Taylor	60
St. Louis, Mo.—	
St. Louis Academy of Sciences	172
Salem, Mass.—	
Prof. J. S. Russell	1
Toronto, Can.—	
Canadian Institute	6
Utica, N. Y.—	
State Lunatic Asylum	4
Washington, D. C.—	
United States Coast Survey	672
United States National Observatory	149
United States Patent Office	465
Superintendent of Census	200
J. E. Hilgard	15
Windsor, Nova Scotia—	
Prof. H. How	18
	3,316

D.

Addressed packages received by the Smithsonian Institution from Europe, for distribution in America, in 1863.

	No. of packages.
ALBANY, NEW YORK.	
Albany Institute	2
Albany Library	1
Dudley Observatory	6
New York State Agricultural Society	29
New York State Library	21
New York State Medical Society	1
New York State University	4
Dr. E. Emmons	1
Professor James Hall	4
AMHERST, MASSACHUSETTS.	
Amherst College	4
Dr. E. Hitchcock	2
Charles H. Hitchcock	1
Professor Charles Upham Shepherd	2
ANNAPOLIS, MARYLAND.	
State Library	4
ANN ARBOR, MICHIGAN.	
Observatory	2
Dr. Brunnow	4
AUGUSTA, MAINE.	
State Library	4
BALTIMORE, MARYLAND.	
Maryland Historical Society	2
Peabody Institute	1
Dr. J. J. Graves	1
Dr. John G. Morris	4
P. R. Uhler	2
BOSTON, MASSACHUSETTS.	
American Academy of Arts and Sciences	86
Boston Society of Natural History	65
Bowditch Library	1
Geological Survey of Massachusetts	3
Massachusetts Historical Society	3
New England Historico-Genealogical Society	1
North American Review	1
Perkins' Institute for the Blind	1
Prison Discipline Society	1
Public Library	4
State Library	4
Dr. S. L. Abbott	1
Dr. T. W. Harris	1
BOSTON, MASS.—Continued.	
Professor C. J. Jackson	1
Professor Rogers	1
S. H. Scudder	1
Charles Sprague	1
George Ticknor	1
BRATTLEBORO', VERMONT.	
Asylum for Insane	1
BRUNSWICK, MAINE.	
Historical Society of Maine	2
BURLINGTON, IOWA.	
Iowa Historical and Genealogical Institute	2
BURLINGTON, VERMONT.	
University of Vermont	1
CAMBRIDGE, MASSACHUSETTS.	
American Association for Advancement of Science	21
Astronomical Journal	2
Harvard College	11
Observatory of Harvard College	21
Professor L. Agassiz	40
G. P. Bond	4
Professor H. J. Clark	1
Professor J. P. Cooke	1
Dr. John Dean	1
Hon. Edward Everett	1
Dr. B. A. Gould	8
Professor Asa Gray	8
Professor H. W. Longfellow	1
Professor J. Lovering	1
Professor Jules Marcou	2
Professor B. Peirce	8
F. W. Putnam	1
Dr. T. H. Safford	2
Professor G. A. Schmit	1
Mr. Tuttle	1
CHICAGO, ILLINOIS.	
Academy of Sciences	6
Mechanics' Institute	1
CINCINNATI, OHIO.	
Astronomical Observatory	7
Dental Register of the West	1
Historical and Philosophical Society of Ohio	1
Mercantile Library	1
John G. Anthony	1

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	No. of packages.
CLINTON, NEW YORK.	
Observatory of Hamilton College	1
Dr. C. H. F. Peters	2
COLUMBIA, MISSOURI.	
Geological Survey of Missouri	8
COLUMBIA, PENNSYLVANIA.	
Professor S. S. Haldeman	1
COLUMBUS, OHIO.	
Ohio State Board of Agriculture	51
State Library	4
Leo Lesquereux	2
CONCORD, NEW HAMPSHIRE.	
New Hampshire Historical Society	1
State Library	4
DES MOINES, IOWA.	
State Library	21
DETROIT, MICHIGAN.	
Michigan State Agricultural Society	15
Lieutenant Colonel J. D. Graham	6
Dr. Tappan	1
DORCHESTER, MASSACHUSETTS.	
Dr. Edward Jarvis	1
EAST GREENWICH, NEW YORK.	
Asa Fitch	3
EASTON, PENNSYLVANIA.	
Prof. J. H. Coffin	1
FRANKFORT, KENTUCKY.	
Geological Survey of Kentucky	10
State Library	4
GAMBIER, OHIO.	
Kenyon College	5
GEORGETOWN, D. C.	
Georgetown College	5
Dr. H. King	1
A. Schott	2
HARRISBURG, PENNSYLVANIA.	
State Library	6
State Lunatic Asylum	1
HARTFORD, CONNECTICUT.	
Historical Society of Connecticut	1
State Library	4
HAVANA, CUBA.	
Royal Economical Society	2
HUDSON, OHIO.	
Western Reserve College	4
Professor Charles A. Young	1
INDIANAPOLIS, INDIANA.	
Indiana Historical Society	1
State Library	4
IOWA CITY, IOWA.	
State University	13
JACKSONVILLE, ILLINOIS.	
Institution for the Blind	1
JANESVILLE, WISCONSIN.	
State Institution for the Blind	3
JEFFERSON CITY, MISSOURI.	
Historical Society of Missouri	1
State Library	4
LANCASTER, OHIO.	
Dr. J. M. Bigelow	1
LANSING, MICHIGAN.	
State Agricultural College	1
State Library	4
LEON, NEW YORK.	
T. Apoleon Cheney	2
LITTLE ROCK, ARKANSAS.	
State Library	3
LOUISVILLE, KENTUCKY.	
Colonel Long	1
Professor J. Lawrence Smith	1
Dr. L. P. Yandell	1

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	No. of packages.
MADISON, WISCONSIN.	
Historical Society of Wisconsin	2
Skandinaviske Presse-Forening	1
State Agricultural Society	16
State Library	9
MONTPELIER, VERMONT.	
Historical and Antiquarian Society of Vermont	1
State Library	6
Albert Hager	1
MONTREAL, CANADA EAST.	
Natural History Society	11
Professor Billings	2
Thomas E. Blackwell	1
Professor J. W. Dawson	3
Sir W. Logan	2
Professor T. S. Hunt	1
M. Toly de Lotbinière	1
NEW BRUNSWICK, NEW JERSEY.	
Geological Survey of New Jersey	2
Professor George H. Cook	3
NEW HAVEN, CONNECTICUT.	
American Journal of Science and Arts	32
American Oriental Society	14
Yale College	1
Professor J. D. Dana	18
Professor E. Loomis	5
Professor B. Silliman	21
Professor W. D. Whitney	4
NEW ORLEANS, LOUISIANA.	
New Orleans Academy of Natural Sciences	13
NEW YORK, N. Y.	
American Agriculturist	1
American Ethnological Society	2
American Geographical and Statistical Society	29
American Institute	1
Astor Library	5
Farmer and Mechanic	1
Historical Society	2
Journal of Pharmacy	1
Medical College	1
New York City Lunatic Asylum	1
New York Dental Journal	1
New York Lyceum of Natural History	53
NEW YORK, N. Y.—Continued.	
University	1
W. Cooper	1
Dr. Draper	3
Dr. Daniel Eaton	2
T. Egleston	1
D. G. Elliot	1
T. W. Greene	1
Dr. Harper	1
C. F. Jung	2
G. N. Lawrence	2
Charles B. Norton	1
Edward Norton	3
Baron Ostensacken	7
Temple Prime	3
John H. Redfield	1
James Renwick	1
Dr. John Torrey	1
Mr. Wheatley	1
NORTHAMPTON, MASSACHUSETTS.	
Mr. Lyman	7
OLYMPIA, WASHINGTON TERRITORY.	
Territorial Library	2
OMAHA, NEBRASKA.	
State Library	5
OWEGO, NEW YORK.	
Mr. Pumpelly	5
PEORIA, ILLINOIS.	
Dr. Brendel	1
PHILADELPHIA, PENNSYLVANIA.	
Academy of Natural Sciences	114
American Philosophical Society	56
Central High School	1
Dental Cosmos	1
Entomological Society	4
Franklin Institute	14
Historical Society of Pennsylvania	3
Institution for the Blind	3
Wagner Free Institute	3
Dr. Allen	1
A. D. Brown	1
Lorin Blodget	1
J. Cassin	1
F. Clay	1
T. A. Conrad	1
Dr. E. D. Cope	4
E. T. Cresson	1

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	No. of packages.
PHILADELPHIA, PA.—Continued.	
E. Durand	2
Professor Haldeman	2
Dr. Isaac Lea	12
Dr. J. L. Le Conte	6
Dr. Joseph Leidy	8
H. Norton	1
G. Ord	1
William Sharswood	4
H. S. Tanner	1
George W. Tryon	1
Professor Wagner	1
Horatio C. Wood	1
PRINCETON, NEW JERSEY.	
Professor A. Guyot	6
PROVIDENCE, RHODE ISLAND.	
Rhode Island Historical Society	2
State Library	4
Professor A. Caswell	2
QUEBEC, CANADA EAST.	
Astronomical Observatory	1
Laval University	1
Literary and Historical Society	2
QUINCY, ILLINOIS.	
Dr. John Ritter	1
ROCK ISLAND, ILLINOIS.	
Dr. Velie	1
Benjamin D. Walsh	2
RYE, NEW YORK.	
John C. Jay	1
SACRAMENTO, CALIFORNIA.	
State Library	3
ST. LOUIS, MISSOURI.	
Deutsche Institute für Förderung der Wissenschaften	1
St. Louis Academy of Sciences	86
St. Louis University	1
Dr. George Bernays	1
Dr. George Engelmann	5
Dr. Adam Hammer	1
Dr. B. F. Shumard	10
John Wolf	1
ST. PAUL, MINNESOTA.	
Historical Society of St. Paul	2
SALEM, MASSACHUSETTS.	
Essex Institute	2
SAN FRANCISCO, CALIFORNIA.	
California Academy of Natural Sciences	25
Professor W. P. Blake	4
SANTIAGO, CHILI.	
University	3
SPRINGFIELD, ILLINOIS.	
State Agricultural Society	1
State Library	4
STOCKTON, CALIFORNIA.	
State Lunatic Asylum	1
TORONTO, CANADA WEST.	
Bureau of Agriculture and Statistics	1
Canadian Institute	7
Magnetical and Meteorological Observatory	1
University College	1
TOPEKA, KANSAS.	
State Library	6
TRENTON, NEW JERSEY.	
State Library	4
UTICA, NEW YORK.	
State Lunatic Asylum	1
WASHINGTON, D. C.	
Bureau of Ordnance and Hydrography	1
Library of Congress	4
National Observatory	83
Navy Department	3
Ordnance Bureau	2
Revenue Department	1
Secretary of State	1
Surgeon General United States Army	3
Topographical Bureau	1
United States Coast Survey	23
United States Patent Office	120

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	No. of packages.		No. of packages.
WASHINGTON, D. C.—Continued.		WASHINGTON, D. C.—Continued.	
War Department	6	T. Pösche	1
Colonel J. G. Abert	4	Captain John Rodgers	1
Professor A. D. Bache	20	S. W. Simm	1
Dr. E. Coues	1	H. R. Schoolcraft	1
Theo. Gill	1	Dr. W. Stimpson	5
Captain J. M. Gilliss	24	W. A. Treadway	1
General Emory	2	H. Ulke	1
Mr. Glover	1	Baron Von Gerolt	1
Dr. F. V. Hayden	1	Captain Charles Wilkes	2
Professor Hubbard	1	John Xantus	1
General A. A. Humphreys	1		
Colonel S. H. Long	1	WORCESTER, MASSACHUSETTS.	
D. Smith McCauley	1		
Professor G. A. Matile	1	American Antiquarian Society	4

Total of addresses	273
Total of parcels	1,522

MUSEUM AND COLLECTIONS.

It is gratifying to be able to state that the interest in the subject of natural history, which received so material a check in 1861, and showed symptoms of revival in 1862, has continued to manifest itself still more strongly during the year 1863. No better indication of this could be found than in the increase in the number of collections received by the Institution, which amounted to 264 distinct donations in 1863, while, in 1862, there were but 124.

Among the collections received have been many specimens of great interest; some, the results of special explorations under the auspices of the Institution for developing the natural history of portions of this continent; others, the spontaneous offerings of correspondents; and others, again, exchanges received in return for donations of specimens on the part of the Institution. No additions have been made by purchase, the Institution not having funds at its command for this purpose. It has, nevertheless, been found that a given amount of money can be better applied in meeting the expenses of explorations in particular regions than in buying collections already made. The results thus obtained are usually more varied in their character, and more important, from having been accomplished under definite instructions, and with special reference to the acquisition of facts and information additional to that which would be furnished by the specimens themselves. It is not merely specimens of natural history that are secured in the course of the several explorations, but information is obtained respecting the habits of animals, the ethnological peculiarities of human races, the meteorology, the physical geography, the geology of the country, &c.

EXPLORATIONS.

Among the explorations wholly or partially carried on under the auspices of the Smithsonian Institution, and furnishing results of more or less interest, may be mentioned the following:

Explorations by Mr. Kennicott.—A brief mention was made in the last report of the return of Mr. Kennicott, late in 1862, after an absence of nearly four years in the north, his movements while there having previously been indi-

cated in the reports of 1859, 1860, and 1861. By the arrival of all his collections, and those of gentlemen connected with the Hudson's Bay Company, who have so liberally aided him and the Institution in the effort to develop a knowledge of the natural and physical history of the north, we are now enabled better to realize the magnitude of the results of these operations. The collections received in 1863 (which include some which should have arrived in the end of 1862) filled forty boxes and packages, many of them of large size, and weighing, in the aggregate, about 3,000 pounds. They embraced thousands of skins of birds and mammals, eggs of nearly all the birds nesting in the north, numerous skulls and skeletons of animals, fishes in alcohol and preserved dry, insects, fossils, plants, &c.

Not in any way inferior in interest and importance to the natural history collections were those relating to the ethnological peculiarities of the Esquimaux and different tribes of Indians inhabiting the Arctic regions. It is believed that no such series is elsewhere to be found of the dresses, weapons, implements, utensils, instruments of war and of the chase, &c., &c., of the aborigines of Northern America.

The cataloguing and labelling of the specimens last received is now nearly completed, and Mr. Kennicott will then proceed to make a detailed report of the scientific results of his operations, as well as those of the various gentlemen of the Hudson's Bay service who co-operated in the work. The materials at his command will serve to fix with precision the relationships of the arctic animals to those of more southern regions, their geographical distribution, their habits and manners, and other particulars of interest, and to extend very largely the admirable records presented by Sir John Richardson relative to arctic zoology.

The Institution has already acknowledged, in many ways, its indebtedness to the Hudson's Bay Company, as well as to its officers, for their numerous favors—the company itself, through its secretary, Mr. Thos. Fraser, of London; the governors, Sir George Simpson and Mr. Dallas; Mr. E. M. Hopkins, the secretary at Montreal; the chief factors, Governor Wm. McTavish, Mr. George Barnston, Mr. John McKenzie, Mr. J. A. Grahame, Mr. Wm. Sinclair; the chief traders, Mr. B. R. Ross, Mr. W. L. Hardisty, Mr. R. Campbell, Mr. Jas. Lockhart, and others, together with Mr. R. W. MacFarlane, Mr. L. Clarke, Mr. S. Jones, Mr. J. S. Onion, the Rev. W. W. Kirkby, Messrs. Andrew and James Flett, Mr. C. P. Gaudet, Mr. John Reid, Mr. Harriot, and others—all have lent their aid towards the accomplishment of the work—every possible facility was given to Mr. Kennicott, every privilege granted within the rules of the company. At all the posts he was an honored guest, and he and his collections and outfit were transported from point to point in the company's boats and sledges without charge.

In addition to collections from the region traversed by Mr. Kennicott in his four years' exploration, some valuable specimens have been received from other points of British North America. Conspicuous among these is a series of birds and eggs from Rigolette, in Labrador, gathered by Mr. Henry Conolly, of the Hudson's Bay Company's service, and brought to Boston, without charge, by Mr. J. W. Dodge. This collection embraced specimens of the rare Labrador falcon, and others of much interest. A collection of birds and other objects of natural history, made at Moose Factory, for the Institution, by Mr. John McKenzie, has reached London by ship from Hudson's Bay, and may shortly be expected in Washington.

Exploration of Western Mexico by Mr. Xantus.—In my last report I mentioned that Mr. John Xantus, so long and so well known in connexion with explorations about Fort Riley, Kansas, Fort Tejon, California, and Cape St. Lucas, was about proceeding to a new field of operations. He left New York on the 11th of December, 1862, for Manzanillo, Mexico, the Panama Railroad

Company and the Pacific Mail Steamship Company, with that liberality they have so steadily exhibited in their transactions with the Institution, having given free passage over their respective routes to himself and his outfit. Mr. Xantus arrived at Manzanillo early in January, 1863, and making this and Colima his principal points of departure, extended his explorations in various directions, especially among the mountain regions. He is still occupied in his labors, the field being very extensive and of varied interest. Many of his collections have already been received, and found to contain numerous species of birds, reptiles, fishes, shells, &c., new to science, while others throw much light on the geographical distribution of the plants and animals of Mexico and Central America.

Explorations in Costa Rica.—For some time past much attention has been directed by naturalists toward the natural history of Costa Rica, a region which, from its peculiar physical conformation, indicated a fauna quite different from that of the adjacent states. The birds were particularly sought after owing to the many remarkable forms, brought to light by travellers. It was, therefore, with no little gratification that a collection of birds, made by Dr. A. Von Frantzius, an eminent naturalist and physician, resident in Costa Rica, aided by the Hon. C. N. Riotte, United States minister, and Mr. J. Carniol, was received a few months ago at the Institution. A careful examination of these specimens proved that the peculiar interest of the fauna had not been overestimated, a large proportion of the species being either new, or but recently described. Additional collections, shortly expected from Dr. Von Frantzius, will, it is hoped, increase still more our knowledge of the species.

Miscellaneous explorations in Mexico.—For several years past a highly valued meteorological correspondent of the Institution, Dr. Charles Sartorius, of Mirador, has made contributions of specimens of the natural history of his vicinity. During the year several collections were received from him of much interest and importance, especially certain species of Mexican deer, recently described, and but little known. As Dr. Sartorius, aided by his son, Mr. Florentin Sartorius, is now engaged in preparing an account of the animals of eastern Mexico, with special reference to their habits, &c., it is a source of gratification to us to have it in our power to aid him by identifying the species from his specimens, which his remoteness from large collections and libraries prevents him from doing for himself. Prof. F. Sumichrast, of Orizaba, has also made valuable contributions of birds and mammals of Mexico, and proposes to renew these whenever the condition of the internal affairs of Mexico will allow of the transmission of his collections. Dr. G. Berendt, of Tabasco, is also occupied in a similar manner in the interest of science and of the Institution.

Explorations in Guatemala and the west coast of Central America.—Mr. Osbert Salvin, an eminent English ornithologist, who has spent many years in the exploration of Guatemala, has transmitted to the Institution a second collection of the birds of that region. As these contain specimens of most of his new species, and all have been carefully compared, as far as practicable, with the types, his series of birds is of especial value, as furnishing standards for the identification of other collections.

Additional collections of much interest continue to be sent to the Institution by Captain J. M. Dow, of the Panama Railroad Company, so frequently mentioned in my previous reports. Certain rare birds and fishes collected by him are especially noteworthy.

Trinidad.—A collection of nearly fifty species of birds of Trinidad was presented by Mr. Galody, United States consul at Antigua, embracing many species not formerly in possession of the Institution.

Jamaica.—Mr. W. T. March, from whom the Institution has already received extensive collections in Jamaican zoology, has again made an important contri-

bution of an extensive series of birds' nests and eggs, the materials upon which he based a memoir on the birds of Jamaica, transmitted to the Institution, to be published by the Philadelphia Academy of Natural Sciences, and printed in its proceedings for November, 1863.

Cuba.—Additional collections were received during the year from Mr. Charles Wright and Professor F. Poey, embracing new and rare species of birds, shells, reptiles, and fishes. Some collections, transmitted by Dr. J. Gundlach, have not yet reached us.

Ecuador.—The Hon. C. R. Buckalew, now United States senator, while United States minister, resident at Ecuador, made quite an extensive collection of the birds of that country, which he has lately presented to the Institution. Nearly all of the species thus obtained were new to the cabinet.

No collections of magnitude, from regions or localities other than American, have been received during the year. It is not the intention or expectation of the Institution to make general collections of the natural history of the globe, neither its space nor available funds warranting so broad a field of operations. By limiting its labor to America, a hope may be entertained of possessing, in time, a complete series of the animals of the continent.

Exotic collections, as far as they are spontaneously offered, and especially such as are necessary to illustrate the characters of American species, are always acceptable, and the specimens gathered by the government exploring expeditions, of which the Smithsonian Institution is the custodian, will always be carefully preserved; but any especial efforts towards the increase of the museum may advantageously be confined, as a general policy, to the New World.

The most important additions, it will be readily seen, relate to the class of birds. Desirous of extending the observations upon the birds of North America, as published in the ninth volume of the Pacific railroad report, a circular was issued by the Institution, which has been distributed by the State Department to the consular and diplomatic officers of the United States in the foreign portions of America, asking aid in completing the collection of birds; and important additions are expected from the request thus extended. The materials received will be used, in connexion with those already in possession of the Institution, in the preparation of catalogues and monographs relative to American ornithology.

Among the specimens received by the Institution during the year should especially be mentioned the great Ainsa or Tucson meteorite.

This meteorite was first discovered by the Jesuit missionaries in Sonora, by whom it was considered a great curiosity, exciting much speculation as to its origin. In 1735 the "Gran Capitan de las Provincias del Occidente, Don Juan Baptista Anza, was induced to visit the ærolite," and found it at a place called "*Los Muchaches*," in the Sierra Madre, and, struck with its appearance, undertook to transport it to San Blas, then the nearest port of entry, with the view of carrying it to Spain. With this object it was brought as far as the Presidio, near Tucson, in Arizona, and left there on account of the difficulty of carrying it any further. After the withdrawal of the Spanish garrison it was taken into the town of Tucson, set up vertically, and used as a kind of public anvil, of which it bears marks at the present time. In this condition it was seen and reported upon by various travellers; among others it was visited by John R. Bartlett, July 18, 1852, at the time Commissioner of the United States and Mexican Boundary Survey. Mr. Bartlett gives a short account of it, (Personal Narrative, volume II, p. 297,) accompanied by a figure, (the lower one on the plate,) where it is represented as resting upon two legs, owing to the lower part of the ring, of which it consists, being buried in the ground. His estimate of six hundred pounds as its weight falls far within the actual amount.

In 1857, Dr. B. J. D. Irwin, United States army, then stationed at Fort Buchanan, south of Tucson, found this meteorite lying in one of the by streets of the village, half buried in the earth. As no one claimed it, he publicly announced his intention to take possession of it and forward it to the Smithsonian Institution, whenever an opportunity offered. Some time after, assisted by Mr. Palatine Robinson, of Tucson, (near to whose house the meteorite lay,) he succeeded in having it sent, by the agency of Mr. Augustine Ainza, to Hermosillo, where it remained for some time at the hacienda of Don Manuel Ynigo, father-in-law of Mr. Ainza.

In May, 1863, Mr. Jesus Ainza, brother of Mr. Augustine Ainza, and grandson of Doña Ana Ainza de Iglas, the daughter of Don Juan Bautista Ainza, visited Sonora, and on his return brought the meteorite with him to San Francisco, where it was delivered by his brother, M. Santiago Ainza, to the agent of the Smithsonian Institution, Mr. A. B. Forbes, of the Pacific Mail Steamship Company, and forwarded by him, *via* the Isthmus, to Washington, where it arrived in November, and is now on exhibition, and the great object of attraction to visitors in the Smithsonian hall. It is proper to state that, although Dr. Irwin was authorized to expend whatever was necessary to secure the transmission of the meteorite to San Francisco, beyond some small expenses paid by him for placing it upon the truck in Tucson, no charge was made by the Ainza family for the cost of transportation to Guaymas and delivery to the Pacific Mail Steamship Company, performed partly with their own wagons and partly by other means of conveyance. It was brought free of charge from Guaymas to San Francisco by the Flint and Haliday line of steamers. While on the route to New York the Pacific Mail Steamship Company and the Panama Railroad Company, with that liberality which has ever characterized their intercourse with the Smithsonian Institution, transported it without expense to Aspinwall, and thence to New York.

The meteorite is in the shape of an immense signet ring, much heavier on one side, where it is nearly flat on its outer surface, and presents the face used as an anvil. The greatest exterior diameter is 49 inches; width of thickest part of the ring 9 inches, the least 38 inches; the greatest width of the central opening, 23 inches; width of thickest part of the ring, 17½ inches. The weight is now 1,400 pounds, but some portions have been removed from time to time, probably reducing it considerably. Its composition is principally of iron, with small specks of a whitish silicious mineral diffused through it.

A careful chemical and physical examination of the meteorite will be made by Professor G. J. Brush, of New Haven, to whom the Smithsonian Institution has committed the subject for a detailed report.

As the ærolite was first brought from the mountains north of Tucson by the great grandfather of the gentleman to whose exertions in transporting it to Washington the Institution owes so much, it is proposed to call it the "Ainsa meteorite." To Dr. Irwin, of the United States medical department, the Institution is also under great obligations for his agency in securing this specimen.

Dr. Irwin states that the inhabitants of Tucson have a tradition that a shower of these meteorites took place in the Santa Catarina mountains about two hundred years ago, and that there are many other masses of a similar character yet remaining in those mountains.

This meteorite is among the largest known, and in this country is only exceeded a little in weight by the Gibbs meteorite in the cabinet of Yale College, New Haven, while it surpasses the latter in size, being disposed in the form of a ring instead of a solid mass.

The Smithsonian Institution also possesses the third largest meteorite in the country in the "Couch meteorite," weighing 252 pounds, and brought from Northeastern Mexico by Major General D. N. Couch, and by him presented to the Institution.

IDENTIFICATION OF SPECIMENS.

Continued progress has been made during the year in the determination and arrangement of the species in the Smithsonian collections, and the cabinet is gradually becoming more and more useful for reference and study. Any apparent shortcoming in this respect will be excused in view of the fact that the work done is mainly a voluntary contribution on the part of gentlemen engaged in making special examinations of the Smithsonian collections, and the Institution is under many obligations for their assistance.

DISTRIBUTION OF SPECIMENS.

In accordance with the plan of the Institution, as fast as the identification of the species is satisfactorily accomplished, the duplicate specimens are set aside for distribution to such museums at home and abroad as appear to be suitable recipients. The total number of objects thus distributed to the end of the year 1863, all properly determined and labelled, amounts to 26,651 species, and 50,601 specimens, as shown by the following schedule:

Statement of specimens of natural history distributed by the Smithsonian Institution up to December 31, 1863.

Specimens.	Prior to 1854.		1854 to July, 1861.		1861 to August, 1863.		Total.		Special distribution of shells of Exp'g Exp'n.	
	Species.	Specimens.	Species.	Specimens.	Species.	Specimens.	Species.	Specimens.	Species.	Specimens.
Mammals	5	5	404	624	172	216	581	845	....	....
Birds	825	1,035	3,162	4,255	1,787	2,494	5,774	7,784	....	....
Reptiles	18	22	1,470	2,356	18	18	1,506	2,396	....	....
Fishes	....	....	1,623	3,921	20	28	1,643	3,949	....	....
Crustaceans	....	....	936	1,894	....	....	936	1,894	....	....
Radiates	....	....	551	727	....	....	551	727	....	....
Mollusks	....	....	588	1,985	310	380	898	2,365	10,934	44,112
Invertebrates, insects, &c.	....	....	216	330	312	400	528	712	....	....
Eggs of birds	114	307	1,537	3,558	628	1,235	2,279	5,100	....	....
Fossil invertebrates	....	....	....	....	747	2,238	747	2,238	....	....
Fossil vertebrates	....	....	....	....	....	....	....	....	....	....
Skulls	58	58	....	....	5	5	63	63	....	....
Minerals and rocks	....	....	....	....	211	354	211	354	....	....
Total	1,020	1,527	10,487	19,650	4,210	7,368	26,651	72,657	10,934	44,112

In the index to the three volumes of transmissions of specimens for examination, or donation, the names of two hundred and fifty-nine institutions and individuals are entered up to August, 1863.

N. B.—The preceding enumeration of specimens distributed does not include the specimens (duplicates) retained by collaborators in behalf of certain authorized collections—as of insects, by Messrs. Leconte, Uhler, Morris, Ostensacken, Saussure, Edwards, Hagen, Loew, Scudder, &c.; of vertebrate fossils, by Leidy, for the Philadelphia Academy; of fishes, by Professor Agassiz; shells, by Messrs. Carpenter, Binney, Tryon, &c.; mammals, by Messrs. Leconte, Allen. &c.; birds, by Mr. Cassin; reptiles, by Mr. Cope; plants, by Messrs. Torrey, Gray, Engelman, and Eaton. These will probably amount to at least 10,000 species, and 20,000 specimens additional.

The cataloguing of specimens in the record-books of the Institution has been continued during the year, and, as will be seen by the accompanying table, now amounts to 86,547 entries, being an increase, since 1863, of over 12,000.

Table showing the total number of entries on the record-books of the Smithsonian collection at the end of the years 1861, 1862, *and* 1863.

	1861.	1862.	1863.
Skeletons and skulls	4,459	4,750	6,275
Mammals	5,550	5,900	7,175
Birds	23,510	26,157	31,800
Reptiles	6,088	6,311	6,325
Fishes	3,643	4,925	5,075
Eggs of birds	4,830	6,000	7,275
Crustaceans	1,287	1,287	1,287
Mollusks	9,718	10,000	10,450
Radiates	1,800	2,675	2,725
Fossils	1,031	2,100	2,550
Minerals	3,500	3,725	4,925
Ethnological specimens	550	825	875
Annelids	105	109	110
Total	66,075	74,764	86,847

LIST OF DONATIONS TO THE MUSEUM OF THE SMITHSONIAN INSTITUTION IN 1863.

Atkins, L. S.—Eggs of birds and shells from Ohio.
Ainsa, J.—See Irwin.
Akhurst, J.—Birds from St. Thomas, West Indies.
Baer, O. P.—Unionidæ from Indiana.
Baird, S. F.—Iron ore from Hanover station; series of skins and eggs of birds, mammals, fishes, and invertebrates, from Wood's Hole and Cohasset, Massachusetts.
Baird, Mrs. S. F.—*Leuciscus*, from Potomac river.
Beadle, Rev. E. R.—Bergen Hill minerals.
Bean, W.—Collection of annelids and cirripeds of Great Britain.
Behrens, Dr.—Insects from California.
Berlin Museum.—54 skins of birds of Central and South America.
Bethune, Rev. C. S.—Skin of *Scalops breweri*, Canada.
Blackman, Mr.—Skins and eggs of birds, Illinois.
Blake, W. P.—Keg of fishes from Hakodadi, Japan.
Bland, Thomas.—*Spiraxis*, from West Indies.
Boardman, George A.—Embryo Canada grouse in alcohol; skins and eggs of birds.
Bouvè, Thomas T.—Large crystals of beryl.
Brass, W.—Birds, mammals, &c., Fort Halkett.
Brevoort, J. C.—Fresh specimen of *Zoarces anguillaris.*
Bruckart, H. G.—Insects from Lancaster county, Pennsylvania.
Buckalew, Hon. C. R.—Collection of birds of Ecuador.
Burling, W.—Skin of *Haliætus pelagicus* from the Amoor river. (Through Samuel Hubbard.)
Carniol, J.—Skins of Costa Rican birds.
Carpenter, P. P.—Fossils from vicinity of Moscow.
Carpenter, Robbie S.—Skin of starling, *Sturnus vulgaris*, Warrington, England.
Clark, Lawrence.—A general zoological collection from Fort Rae, Great Slave lake.
Coleman, Lyman.—Seeds of Damascus thorn; petrified wood from Cairo.
Coleman, W. T.—Birds and eggs from Canada.
Comstock, A.—Cuttings of California grapes.

Conolly, H.—Skins and eggs of birds from Labrador.
Cooper, Dr. J. G.—Shells of California.
Coues, Dr. E.—Series of skins of birds of District of Columbia.
Cowles, P. W.—Insects from Vicksburg.
Crosier, Dr. E. S.—Vorticella, &c., New Albany, Indiana.
De Saussure, Dr. H.—Skins of Mexican birds, and lacustrian antiquities of Switzerland.
Diebitsch, Professor H.—Rana pipiens.
Dodd, P. W.—Skulls of animals and eggs of birds from Sable island.
Dow, Captain J. M.—Skins of mammals, and birds, fishes, &c., from west coast of Central America.
Drew, Dr. F. P.—Collection of reptiles and eggs of birds from Kansas.
Drewsen, Charles.—Series of Greenland shells.
Drexler, C.—Series of skins of birds of the District of Columbia.
Egleston, Thomas.—Series of European fossils.
Elliot, D. G.—Skins of European gulls; skins of humming birds.
Elliot, H. W.—Large collection of Unionidæ; shells, &c., in alcohol, Ohio.
Engelmann, Dr.—Fossils from Illinois.
Fairbanks, Professor.—Box of eggs.
Fay, Joseph S.—Chlorastrolite from Lake Superior.
Flett, Andrew.—Skins and eggs of birds; Fort Normann.
Flett, James.—Eggs of birds, &c., from La Pierre's house.
Foreman, Dr. E.—Five boxes of minerals from Maryland.
Freiburg, Mining Academy of.—Box of mineralogical and geological specimens from Germany.
Frick, Dr.—Shells of California and Japan.
Galody, M.—Skins of birds of Trinidad.
Gaudet, C. P.—Skins and eggs of birds, &c., from Peel's river.
Gibbs, George.—Indian curiosities.
Gilliss, U. S. N., Captain.—Six boxes of microscopic soundings.
Gilpin, Dr. J. B.—Series of shrews and mice of Nova Scotia.
Goldsmith, Dr. M.—Cricket from the Mammoth Cave, Kentucky.
Gould, Dr. A. A.—Forty species of *Melaniadæ.*
Grahame, J. A.—Skins of mammals, &c., Norway House.
Giebel, Dr. C.—Three boxes of insects of Europe, (365 species.)
Gruber, Ferd.—Skins and eggs of birds from California.
Gundlach, Dr. J.—Specimens of *Gundlachia*, Cuba.
Gunn, Donald.—Skins and eggs of birds from Red River settlement and Lake Winipeg.
Haldeman, Professor S. S.—Types of the species of *Melaniadæ* described by him.
Hall, W. F.—Birds and eggs from Massachusetts.
Hamilton, R.—Collection of skins and eggs of birds from Great Whale river, (through Mr. George Barnston.)
Hardisty, W. L.—Birds, mammals, &c., from Fort Liard.
Harris, W. O.—Minerals from Chester county.
Harriot, Mr.—Skins of birds from Fort Anderson.
Hays, Dr. W. W.—Fishes, &c., from Sacramento river.
Hayden, Dr. F. V.—Alcoholic specimens, Beaufort, South Carolina.
Haymond, Dr. R.—*Cypris* from Indiana.
Hepburn, James.—Skins and eggs of birds from the Pacific coast.
Hibbard, Francis.—Lead ore from New Brunswick.
Hibbard, James.—Antimony ores, New Brunswick.
Hitz, R. B. & George.—1,200 eggs, of twelve species of birds, from Northampton county, Virginia, with shells, &c. (See also Stimpson.) Fossils from Aquia creek.

Hoge, Mr.—Skin of boa from the Serapiqui river.
Hope, John.—Eggs of birds, fishes, &c., Great Bear lake.
Hotaling, C. F.—Rock salt from Louisiana.
Hoxie, W.—Insects from Massachusetts.
Hoy, Dr. P. R.—Nests and eggs from Racine.
Hunt, General L. C.—Indian knife, Klamath lake.
Irwin, Dr. W. W., and J. Ainsa.—Meteorite from Tucson, weighing 1,400 pounds.
Jeffreys, Mr.—Box of minerals of Chester county, Pennsylvania.
Jones, Strachan.—Eggs of birds, &c., from the Yukon.
Julian, A. A.—Series of fishes, &c., Sombrero island.
Keep, Rev. Marcus R.—Moose horns from Maine.
Kennedy, Dr. H. W.—Collection of reptiles of Uruguay.
Kennicott, R.—Insects, eggs, &c., from Illinois.
Kennicott, R., and others.—Fifteen boxes, three bales, one keg, and one chest of Arctic collections. Mr. Kennicott's collections principally from the mouth of the Porcupine river, Peel's river, Fort Good Hope, La Pierre's house, Fort Resolution, &c.
Kirtland, Dr. J. P.—Two boxes of western *Unionidæ.*
Krefft, Dr. G., (through W. Cooper.)—Collection of Australian reptiles.
Krider, John.—Mounted hawks.
Lapham, I. A.—*Unionidæ* of Wisconsin.
Lawrence, George N.—Skins of birds from Central America and Panama.
Lea, Isaac.—Box of *Unionidæ,* and one hundred species of *Melaniadæ.*
Lewis, James, Dr.—Large collection of land and fluviatile shells from the interior of New York.
Lockhart, James.—Large series of zoological specimens, principally birds' eggs, from the Yukon; skins of birds, mammals, eggs, &c., from Fort Resolution.
Lykins, W. H. R.—Fossils from Kansas.
MacFarlane, R. W.—A general zoological and ethnological collection from vicinity of the Anderson river, Arctic America.
McGuire, J. C.—Two boxes of *Unionidæ.*
McKenzie, Hector.—Birds' eggs from Red river.
McKenzie, J.—Birds, &c., from Fort Resolution.
McKenzie, Roderick.—Birds' eggs from Lake Manitobah.
McMurray, W.—Birds' eggs from Winipeg river.
MacTavish, Gov. William.—Skins and eggs of birds, &c., from the Red River country.
Mann, William.—Skins of *Pinicola canadensis,* Lake Superior.
March, W. Thomas.—Three boxes of skins, nests and eggs of Jamaican birds.
Meek, F. B.—Series of fossils from New Jersey and Maryland.
Moore, Carleton R.—Double tail of *Limulus.*
Michener, Dr. E.—156 crania of birds, and 54 of mammals; two boxes of minerals.
Onion, J. S.—Plants, eggs, &c., from Fort Good Hope.
Palmer, Dr. E.—Fossils, minerals, &c., Pike's Peak.
Parker, Rev. H. W.—Marine shells, United States, and two boxes of minerals from New Bedford.
Parkinson, D. T.—Skins and eggs of birds, Indian skulls, plants, &c., Fort Crook, California
Philadelphia Academy of Natural Sciences.—Seventy species of *Melaniadæ.*
Piper, Col., (10th regiment New York volunteer artillery.)—Rock specimens and fossil wood from Fort Meigs, near Washington.
Poey, Prof. F.—Collection of bats and *Neuroptera;* fishes from Cuba.
Poole, Henry.—"Cone in cone" in slate. From a shaft sunk in the Harbor Vein coal seam, Little Glacé Bay, Cape Breton.

Prentiss, D. W.—Series of skins of birds of the District of Columbia.
Quackenbush, Leslie R.—Fossils of the Utica slate.
Reed, John.—Skins and eggs of birds from Big Island, Great Slave Lake. One collection through L. Clarke, jr.
Reed, Peter.—*Sorex platyrhinus*, Washington county, New York.
Richards, Thos.—Skins of birds, &c., from Temiscamingue.
Riotte, Hon. C.—Reptiles and insects in alcohol, skins of birds, shells, &c., Costa Rica.
Ritchie, J. P.—Skin and egg of *Buteo pennsylvanicus* from Massachusetts.
Rodgers, Commodore John.—Ethnological collections of the North Pacific Exploring Expedition.
Ross, B. R.—A general zoological collection from Fort Simpson and vicinity.
Rousseau, E.—Box of shells from New York.
Saemann, L.—Box of European minerals.
Salisbury, Dr. S. H.—*Scalops* in alcohol from Fairfield county, Ohio.
Salle, A.—Skins of Mexican birds.
Salvin, O.—Collection of birds of Guatemala, (150 species.)
Sartorius, Dr. C.—Collection of birds, mammals, alcoholic specimens, &c., Mexico.
Schmidt, Dr.—Birds from the vicinity of Washington, collected by the late Chas. F. Schmidt.
Sclater, Dr. P. L.—Skins of Mexican birds.
Simpson, George B.—Copper spear-head, and other relics.
Sitka, Governor of.—Box of crustacea. (Through Mr. Jas. Hepburn.)
Springer, P. M.—Skins and sterna of birds, Illinois.
Stimpson, Dr. W.—Three boxes of marine invertebrates of Great Britain; two of American.
Stimpson, Dr. W. and R. B. Hitz.—Three boxes shells, eggs, &c., Northampton county, Virginia.
Sumichrast, Prof. F.—Mammals and birds of Mexico.
Surgeon General.—Tertiary fossils, Suffolk, Virginia.
Swan, J. G.—Indian curiosities, skins of birds, eggs, shells, fishes, &c., from Puget Sound.
Thomson, J. H—Box of New England shells.
Tolman, J. W.—Skins and eggs of birds of Illinois.
Trumbull, George.—Wavellite from Chester county.
Tryon, G. W.—One hundred and twenty-five species of *Melaniadæ*.
Ulke, H.—Skins of birds from Illinois.
Van Cortlandt, Dr. E.—Mammals in alcohol, skins of *Lepidosteus*, &c., from Ottawa.
Frantzius, Dr.—Collection of birds and mammals from Costa Rica.
Velie, Dr. J. W.—Eggs of *Protonotaria citrea*, &c., from Illinois.
Vienna Geologisches Reichs-Anstalt.—Collection of Austrian fossils.
Walker, R. O.—Fishes, shells, skulls, &c., Allegheny county, Pennsylvania.
White, Dr.—Marine shells and skulls of mammals, Isthmus, Panama.
Willis, J. R.—Shells, eggs, and fishes of Nova Scotia.
Williams College Lyceum,—Eggs of Greenland birds.
Wilson, N.—Seeds of plants from Jamaica. (Through Thos. Bland.)
Wingate, J. D.—Box of shells, Bellefonte, Pennsylvania.
Woodworth, Dr. J. M.—Reptiles and insects from Memphis.
Wouton, W. G.—Skins and eggs of birds of Nova Scotia.
Wright, Chas.—Birds, shells, and insects of Cuba.
Wynne, Dr. Jas.—Specimen of sphinx or hawk moth from Central America.
Xantus, John.—Fourteen boxes of mammals, birds, and other animals, plants &c., from Manzanillo, Colima, &c.

LIST OF WORKS PUBLISHED IN 1863.

(155.) Ancient Mining on the Shores of Lake Superior. By Charles Whittlesey. 4to., pp. 32, and one map. (Published April, 1863.)

(146.) Meteorological Observations in the Arctic Seas. By Sir Leopold McClintock, R. N. Made on board the Arctic searching yacht "Fox," in Baffin's Bay and Prince Regent's Inlet in 1857, 1858, and 1859. Reduced and discussed at the expense of the Smithsonian Institution by Charles A. Schott, Assistant United States Coast Survey. 4to., pp. 160, and one map.

A small edition of this work was published in May, 1862, but the final issue, with corrections and additions, took place in 1863.

(166.) Records and Results of a Magnetic Survey of Pennsylvania and parts of adjacent States in 1840 and 1841, with some additional Records and Results of 1834, 1835, 1843, and 1862, and a map. By A. D. Bache, LL.D., F. R. S., Member of Corresponding Academy of Sciences, Paris; President of National Academy of Sciences; Superintendent United States Coast Survey. 4to., pp. 88, and one map. (Published October, 1863.)

(169.) Researches upon the Anatomy and Physiology of Respiration in the Chelonia. By S. Weir Mitchell, M. D., and George R. Morehouse, M. D. 4to., pp. 50. (Published April, 1863.)

(156.) Catalogue of Minerals, with their Formulas, &c. Prepared for the Smithsonian Institution by T. Egleston. 8vo., pp. 42.

(140.) List of the Coleoptera of North America. Prepared for the Smithsonian Institution by John L. Leconte, M. D. Part I. 8vo., pp. 60. (Published March, 1863.)

(167.) New Species of North American Coleoptera. Prepared for the Smithsonian Institution by John L. Leconte, M. D. Part I. 8vo., pp. 94. (Published March, 1863.)

(142.) Bibliography of North American Conchology previous to the year 1860. Prepared for the Smithsonian Institution by W. G. Binney. Part I. American authors. 8vo., pp. 658. (Published March, 1863.)

(171.) Monograph of the Diptera of North America. Prepared for the Smithsonian Institution by H. Loew. Part II. Edited by R. Ostensacken. 8vo., pp. 340. (Published January, 1864.)

(160.) Instructions relative to the Ethnology and Philology of America. Prepared for the Smithsonian Institution by George Gibbs. 8vo., pp. 36. (Published March, 1863.)

(161.) A Dictionary of the Chinook Jargon or Trade Language of Oregon. Prepared for the Smithsonian Institution by George Gibbs. 8vo., pp. 60. (Published March, 1863.)

Systematic index to the list of foreign correspondents of the Smithsonian Institution, corrected to January, 1862. 8vo., pp. 16.

Appendix to the list of foreign correspondents of the Smithsonian Institution, corrected to January, 1863. 8vo., pp. 7.

(170.) Comparative Vocabulary. Reprinted from the Smithsonian Instructions relative to ethnology and philology. 4to., pp. 20. (Published May, 1863.)

WORKS STILL IN PRESS.

(174.) Bibliography of North American Conchology. By W. G. Binney. Part II. 8vo., 239 pages stereotyped.

(143.) Synopsis of Air Breathing Shells. By W. G. Binney. 8vo.

(144.) Synopsis of North American Vivipara, &c. By W. G. Binney. 8vo.

(145.) Monograph of American *Corbiculadæ*. By Temple Prime. 8vo., (42 pages in type.)

(177.) Check-list of North American Fossils; cretaceous formation. By F B. Meek. 8vo.

(172.) Palæontology of the Upper Missouri. By F. B. Meek and F. V. Hayden. 4to.

(165.) Monograph of North American Bats. By Harrison Allen, M. D. 8vo.

(173.) On the Microscopic Structure of the Medulla Oblongata and the Trapezium. By Dr. John Dean. 4to.

(175.) Discussion of the Magnetic and Meteorological Observations of Girard College. By Prof. A. D. Bache. Part VII, VIII, IX. 4to.

(179.) List of publications of learned societies, periodicals, and encyclopædic works in the library of the Smithsonian Institution, July 1, 1863.

(178.) Monograph of North American Hymenoptera. By H. De Saussure. Part I. Edited by Edward Norton. 8vo.

LIST OF METEOROLOGICAL STATIONS AND OBSERVERS

OF THE

SMITHSONIAN INSTITUTION

FOR THE YEAR 1863.

Name of observer.	Station.	North latitude.	West longitude.	Height.	Instruments.*	No. of months received.
BRITISH AMERICA.		° ′	° ′	*Feet.*		
Acadia College	Wolfville, Nova Scotia	45 06	64 25	95	A	5
Baker, J. C	Stanbridge, Canada East	45 08	73 00		T	12
Clarke, Lawrence, jr	Fort Rae, Great Slave Lake				T	6
Connolly, Henry	Rigolet, Labrador				B. T	2
Delaney, Edward M. J	Colonial Building, St. John's, Newfoundland.	47 35	52 40	170	B. T. R	4
Everett, Prof. J. D	King's College, Windsor, Nova Scotia.	44 59	64 07	200	A	6
Flett, Andrew	Fort McPherson, Hudson's Bay Territory.	68 00	135 00	200	T	1
Hall, Archibald, M. D	Montreal, Canada East	45 30	73 36	57	A	6
McFarlane, R	Fort Anderson	68 30	127 30			4
	Fort George				T	4
Magnetic Observatory	Toronto, Canada West	43 39	79 21	†108	A	12
Murdock, G	St. John, New Brunswick				B. T. R	7
Phillips, H	Niagara, Canada West	43 09	79 20	270	A	1
Rankin, Colin	Michipicoton, Canada West	47 56	85 06		B. T	12
Richards, Thomas	Kenogumissee, Hudson's Bay Territory.	49 50	84 00	1,000	T	4
MEXICO.						
Laszlo, Charles	San Juan Bautista, Tabasco	17 47	92 36	40	A	8
Sartorius, Dr. Charles	Mirador, Vera Cruz	19 15	96 25	3,600	A	11
CENTRAL AMERICA.						
Riotte, C. N	San José, Costa Rica	9 54	84 06	3,772	T. R	12
White, William T., M. D	Aspinwall	9 21	79 54		A	12
WEST INDIES.						
United States Consul	Turk's Island					3
Julien, Alexis A	Sombrero Island	18 55	63 27	45	A	12
BERMUDA.						
Royal Engineers, (in the Royal Gazette.)	Centre Signal Station, Saint George's				A	12
SOUTH AMERICA.						
Hering, C. T	Government Plantation Rustenberg, colony of Surinam, Dutch Guiana.				A	9

* A signifies Barometer, Thermometer, Psychrometer, and Rain Gauge.
B signifies Barometer.
T signifies Thermometer.
P signifies Psychrometer.
R signifies Rain Gauge.
N signifies no instrument.
† Above Lake Ontario.

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
CALIFORNIA.			° ′	° ′	Feet.		
Ayres, W. O., M. D	San Francisco	San Francisco	37 48	122 27	130	A	8
Belcher, W. C	Marysville	Yuba	39 29	121 30	80	B. T. R.	8
Croft, Charles	Sacramento	Sacramento	38 31	121 29	65	T. R	1
Dunkum, Mrs. Elizab'h S.	Honcut	Yuba	39 25	121 30	....	T	1
Logan, Thomas M., M. D.	Sacramento	Sacramento	38 35	121 28	41	A	6
Parkinson, David F	Presidio of San Francisco.	San Francisco	37 48	122 22	....	A	7
Smith, M. D	Spanish Ranche	Plumas	39 56	120 40	3,700	B. T. R.	6
COLORADO.							
Luttrell, James	Montgomery	Park	39 00	106 00	13,000	T	4
CONNECTICUT.							
Case, Jarvis	Canton	Hartford	42 00	73 00	700	T. R	7
Hunt, Rev. Daniel	Pomfret	Windham	41 52	72 23	587	A	12
Johnston, Prof. John	Middletown	Middlesex	41 32	72 39	175	A	12
Learned, Dwight W	Plymouth	Litchfield	41 40	73 03	....	T	12
Leavenworth, D. C	New Haven	New Haven	41 18	72 56	40	B. T	9
Rockwell, Charlotte	Colebrook	Litchfield	42 00	73 06	....	T	12
Yeomans, William H	Columbia	Tolland	41 40	72 42	....	T	12
DAKOTA.							
Williams, Herbert G	Yankton	....	42 51	97 31	....	....	1
DELAWARE.							
Hedges, Urban D., M. D	Wilmington	New Castle	....	....	....	T. R	12
DISTRICT OF COLUMBIA.							
MacKee, Rev. C. B	Georgetown	Washington	38 54	77 03	....	T. R	2
Smithsonian Institution	Washington	Washington	38 53	77 01	60	A	12
FLORIDA.							
Dennis, William C	Key West	Monroe	24 33	81 28	16	B. T. R.	12
IDAHO.							
Collins, W. O	Fort Laramie	....	42 10	104 47	4,472	T	4
Rosseau, M. C	Fort Benton	....	47 49	110 36	2,780	N	3
ILLINOIS.							
Aldrich, Verry	Tiskilwa	Bureau	41 15	89 66	550	N	12
Babcock, E	Riley	McHenry	42 11	88 20	760	T. R	12
Bacon, E. E	Willow Creek	Lee	41 45	88 56	1,040	N	5
Baker, Nathan T	Belleville	St. Clair	38 29	90 06	600	B. T	1
Ballou, N. E., M. D	Sandwich	De Kalb	41 31	88 30	665	T. R	12
Bandelier, Adolphus F., jr.	Highland	Madison	38 45	89 46	....	B. T. P.	12
Blanchard, Orestes A	Elmira	Stark	41 12	90 15	....	T. R	3
Boettner, Gustav A	Chicago	Cook	41 54	89 40	....	B. T	1
Brendel, Frederick, M. D.	Peoria	Peoria	40 43	89 30	460	A	12
Brookes, Samuel	Chicago	Cook	42 00	87 30	....	T	7
Byrne, Arthur M	Chicago	Cook	41 57	87 38	591	B. T	3
Dudley, Timothy	Jacksonville	Morgan	39 30	90 06	676	T. R	12
Grant, John } Grant, Miss Ellen }	Manchester	Scott	39 33	90 34	683	A	12
Griffing, Henry	Hazel Dell	Cumberland	39 00	88 00	....	N	12
Little, J. Thomas	Dixon	Lee	41 45	89 31	....	T	7
Livingston, Prof. Wm	Galesburg	Knox	....	....	....	A	11
Mead, S. B., M. D	Augusta	Hancock	40 10	91 00	*203	T. P. R.	11
Merwin, Mrs. Emily H	Ottawa	La Salle	41 20	88 47	500	T. R	10
Riblet, J. H	Pekin	Tazewell	40 36	89 45	....	B. T. R.	12
Rogers, O. P. and J. S	Marengo	McHenry	42 14	88 38	842	B. T. R.	5
Tolman, James W	Winnebago Depot.	Winnebago	42 17	89 12	900	B. T. R.	12
INDIANA.							
Anderson, Henry H	Rockville	Parke	36 00	87 00	1,100	T. R	11
Burroughs, Reuben	South Bend	St. Joseph	41 39	86 71	600	T. R	9

* Above low-water mark at Quincy.

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
INDIANA—Continued.			° ′	° ′	*Feet.*		
Chappellsmith, John	New Harmony	Posey	38 08	87 50	320	A	12
Crozier, Dr. E. S	New Albany	Floyd	38 02	85 29	....	B. T. P.	9
Dawson, William	Cadiz	Henry	39 55	85 20	1,060	T. R	2
	Spiceland	Henry	39 48	85 18	....	B. T. R.	9
Dayton, James H	South Bend	St. Joseph	41 39	86 07	600	T. R	3
Haines, John	Richmond	Wayne	39 52	84 59	....	B. T	6
Helm, Thomas B	Logansport	Cass	40 45	86 13	600	T. R	6
Larrabee, William H	Greencastle	Putnam	39 30	86 47	800	N	1
Mayhew, Royal	Indianapolis	Marion	39 55	86 00	698	T. R	12
Rambo, Edward B	Richmond	Wayne	39 47	84 47	800	T. P. R.	2
Redding, Thomas B	Newcastle	Henry	39 55	85 27	1,000	B. T	11
Rice, E. J	Muncie	Delaware	40 12	85 20	....	B. T. R.	3
IOWA.							
Briggs, E. L	Mount Pleasant	Henry	41 00	91 38	....	T. R	1
Chamberlain, John	Davenport	Scott	41 30	90 40	737	A	7
Dunwoody, Wm. P							
Collin, Prof. Alonzo	Mount Vernon	Linn	42 00	91 00	....	T	6
Deering, D. S	Independence	Buchanan	....	....	....	T	5
Doyle, L. H	Waterloo	Black Hawk	42 30	92 31	....	N	1
Farnsworth, P. J., M. D	Lyons	Clinton	41 50	90 10	401	T. R	12
Foster, Suel	Muscatine	Muscatine	41 26	92 00	....	N	9
Gidley, Isaac M	Bangor	Marshall	42 00	93 00	....	T. R	6
Horr, Asa, M. D	Dubuque	Dubuque	42 30	90 52	666	A	12
McConnel, Townsend	Pleasant Plain	Jefferson	41 07	94 51	950	T. R	12
McCoy, Franklin, M. D.	Algona	Kossuth	43 01	94 04	1,500	T	12
McCoy, Miss Elizabeth							
McCready, Daniel	Fort Madison	Lee	40 37	91 28	....	T. R	12
Marshall, Gregory	Vernon Springs	Howard	43 20	92 12	....	B. T. R.	6
Millard, Andrew J	Sioux City	Woodbury	42 33	96 27	1,258	T. R	12
Parvin, Prof. Theodore S	Iowa City	Johnson	41 37	....	621	A	12
Sheldon, Daniel	Forestville	Delaware	42 40	91 50	....	T	4
Townsend, Nathan	Iowa Falls	Hardin	42 32	93 20	....	T. R	2
Walton, Josiah P	Muscatine	Muscatine	41 25	92 02	586	A	1
Wheaton, Alex. Camp	Independence	Buchanan	42 25	92 00	....	T. R	12
KANSAS.							
Browne, O. H	Ridgeway	Osage	39 02	95 11	850	N	3
Drew, F. P., M. D., U. S. A.	Fort Riley	....	39 00	96 30	1,300	T. R	12
Fuller, Arthur N	Lawrence	Douglas	38 58	95 13	970	R	10
Goodnow, Isaac T	Manhattan	Riley	39 13	96 45	1,000	T. R	12
Denison, Henry L							
Soule, W. L. G	Lawrence	Douglas	38 58	95 13	970	R	1
KENTUCKY.							
Matthews, Jos. McD., D. D	Nicholasville	Jessamine	37 58	84 18	940	A	6
Woodruff, E. N	Louisville	Jefferson	38 22	85 38	....	A	1
Young, Mrs. Lawrence	Louisville	Jefferson	38 07	85 24	570	A	12
MAINE.							
Brackett, Geo. Emerson	Belfast	Waldo	44 23	69 08	....	T. R	5
Dana, Wm. D	North Perry	Washington	45 00	67 06	100	A	6
Gardiner, R. H	Gardiner	Kennebec	44 41	69 46	90	B. T. R.	12
Guptill, G. W	Cornishville	York	43 40	70 44	850	T. R	12
Moore, Asa P	Lisbon	Androscoggin	44 00	70 04	130	T. R	12
Parker, J. D	Steuben	Washington	44 41	67 50	50	A	12
Pitman, Edwin	Williamsburg	Piscataquis	....	....	....	T	8
	Sebec						
Pitman, Mark	Foxcroft	Piscataquis	45 12	69 13	....	B. T	6
Reynolds, Lauriston	East Wilson	Franklin	44 44	70 17	....	N	3
Van Blarcom, James	Vassalboro'	Kennebec	44 28	69 47	....	B. T	7
West, Silas	Cornish	York	43 40	70 44	784	T. R	11
Wilbur, Benj. F	Dexter	Penobscot	44 55	69 32	700	T. R	6
	West Waterville	Kennebec	....	....	....	T. R	6
MARYLAND.							
Baer, Miss Harriott M.	Sykesville	Carroll	39 23	76 57	700	T. P. R.	12
Dutton, Prof. J. Russel	Chestertown	Kent	39 12	75 59	....	A	12
Goodman, William R	Annapolis	Anne Arundel	38 59	76 29	20	A	12

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
MARYLAND—Continued.			° ′	° ′	Feet.		
Hansbew, Henry E	Frederick	Frederick	39 24	77 26		A	1
Lowndes, Benjamin O	Bladensburg	Prince George's	38 57	76 58	112	T. R	11
Stephenson, Rev. James	St. Inigoes	St. Mary's	38 10	76 41	45	A	12
MASSACHUSETTS.							
Astronomical Observatory	Williamstown	Berkshire	42 43	73 13	686	B. T. R	7
Bacon, William	Richmond	Berkshire	42 23	73 20	1,190	T. R	6
Barrows, N., M. D	Sandwich	Barnstable	41 45	70 30		T. R	9
Caldwell, John H	Topsfield	Essex				T. R	8
Davis, Rev. Emerson	Westfield	Hampden	42 06	72 48	180	A	12
Dewhurst, Rev. Eli	Baldwinsville	Worcester	42 37	72 05	847	B. T. R	10
Fallon, John	Lawrence	Essex	42 42	71 11	133	A	3
Metcalf, John Geo., M. D	Mendon	Worcester	42 06	71 34		B. T. R	12
Prentiss, Henry C., M. D	Worcester	Worcester	42 16	71 48	528	A	7
Rodman, Samuel	New Bedford	Bristol	41 39	70 56	90	A	11
Snell, Prof. E. S	Amherst	Hampshire	42 22	72 34	267	A	12
MICHIGAN.							
Blaker, Dr. G. H; Bacon, Frank M	Marquette	Marquette	46 33	87 33	620	A	7
Kedzie, Prof. R. C	Lansing	Ingham	42 42	84 35		A	5
Schetterly, Henry R	Northport	Leelenaw	45 23	85 24		R	6
Streng, L. H	Holland	Ottawa	42 00	86 00	680	T. R	12
Van Orden, Wm., jr	Clifton	Keweenaw	47 00	88 00	800	T	5
Whelpley, Miss Florence E	Monroe	Monroe	41 56	83 23	590	T. R	12
Woodard, C. S	Ypsilanti	Washtenaw	42 15	83 47	751	A	10
MINNESOTA.							
Grave, Mary A	Tamarack	Hennepin				T	6
Kelly, O. H	Itasca	Anoka	45 15	93 28	856	T	3
Paterson, Rev. A. B., D. D	St. Paul	Ramsey	44 57	93 05	800	T. R	12
Smith, Henry L	Forest City	Meeker	45 13	94 28		T. R	11
Wieland, C	Beaver Bay	Lake	47 17	91 18	657	B. T	12
MISSOURI.							
Christian, John	Harrisonville	Cass				T	12
Engelmann, George, M. D	St. Louis	St. Louis	38 37	90 15	481	A	7
Fendler, Augustus	St. Louis	St. Louis	38 37	90 16	470	B. T. P	6
Muir, Wm	Laborville	St. Louis	38 33	90 43		T	1
Myers, J. H	Kirksville	Adair	40 38	92 50	1,000	N	2
Ray, George P	Canton	Lewis	40 12	91 37		T	12
Tidswell, Miss Mary Alice	Warrenton	Warren	38 37	91 16	825	T	7
NEBRASKA.							
Bowen, Miss Anna M. J	Elkhorn City	Douglas	41 22	96 12	1,000	T	12
Evans, John	Fontenelle	Washington	41 31	96 45	1,000	T. R	5
Hamilton, Rev. Wm	Bellevue	Sarpy	41 08	95 50		T. R	11
NEW HAMPSHIRE.							
Brown, Branch	Stratford	Coos	44 08	71 34	1,000	T. R	12
Chase, Arthur	Claremont	Sullivan	43 22	72 21	539	B. T. R	12
French, Isaac S., M. D	Loudon Ridge	Merrimack	43 20	71 25	475	T. R	2
Nason, Rev. Elias	Exeter	Rockingham	42 58	70 55	125	B. T	9
Odell, Fletcher	Shelburne	Coos	44 23	71 06	700	B. T	12
Pitman, Charles H	North Barnstead	Belknap	43 38	71 27		T	10
Smith, Rufus	North Littleton	Grafton	44 20	71 50		B. T	3
Whiting, Robert C	Littleton	Grafton	44 20	72 00		T. R	10
NEW JERSEY.							
Beans, Thomas J	Progress	Burlington			30	B. T	12
Brooks, Wm	Passaic Valley	Passaic				T. R	2
Cooke, Robert L	Bloomfield	Essex	40 49	74 08	120	A	1
Deacon, John C	Burlington	Burlington				T	1
Rhees, Morgan J., M. D	Mount Holly	Burlington			30	B. T	12
Stokes, Howard A	Long Branch	Monmouth	40 20	74 06	10	T. R	1
Thompson, George W	New Brunswick	Middlesex	40 30	75 31	90	T	12
Whitehead, W. A	Newark	Essex	40 45	74 10	35	B. T. R	12

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
NEW YORK.			° ′	° ′	*Feet.*		
Arden, Thos. B	Garrison's	Putnam	41 22	74 02	180	T. R	8
Barrows, Storrs	South Trenton	Oneida	43 10	74 56		T. R	2
Bartlett, Erastus B	Vermillion	Oswego	43 26	77 26	327	T	12
Beauchamp, Wm. M	Skaneateles	Onondaga	43 00	76 30	932	B. T	11
Bowman, John	Baldwinsville	Onondaga	43 04	76 41		T	12
Cowing, Philo	Seneca Falls	Seneca	42 54	76 51	463	B. T	4
Dill, John B	Auburn	Cayuga	42 55	74 28		T	12
Denning, William H	Fishkill Landing	Dutchess	41 34	74 18	42	B. T. R	12
Dewey, Prof. Chester; Kreyer, Carl T	Rochester	Monroe	43 08	77 51	516	B. T. R	11
Gregory, S. O	Theresa	Jefferson	41 12	75 48	365	T. R	12
Guest, W. E	Ogdensburg	St. Lawrence	44 43	75 37	279	N	4
Heimstreet, John W	Troy	Rensselaer	42 44	73 37	58	A	7
Holmes, Dr. E. S	Wilson	Niagara	43 20	78 56	250	T	12
House, John C	Waterford	Saratoga	42 47	73 39	70	A	5
Howell, Robert	Nichols	Tioga	42 00	76 32		T	12
Ingalsbe, Greenville M	South Hartford	Washington	43 15	73 21	400	T. R	5
Mack, Rev. Eli T	Flatbush	Kings	40 37	74 02	54	B. T. R	8
McMore, P. A	Fort Ann	Washington	42 39	73 44	1,430	T. R	2
Malcom, Wm. Schuyler	Oswego	Oswego	43 28	76 30	250	B. T. R	12
Mathews, M. M., M. D	Rochester	Monroe	43 08	77 51	525	A	12
Morris, Prof. Oran W	New York	New York	40 43	74 05	25	A	12
Paine, H. M., M. D	Clinton	Oneida	43 03	75 15	600	T. P. R	12
Pratt, Daniel J	Fredonia	Chautauqua	42 26	79 24		B. T. R	1
Roe, Rev. San. W., M. D	Jamestown	Chautauqua	42 06	79 19	1,454	T. R	1
Russell, Cyrus H	Gouverneur	St. Lawrence	44 19	75 29		B. T. R	12
Spooner, Dr. Stillman	Wampsville	Madison	43 04	75 50	500	T. R	12
Swift, Lewis	Marathon	Cortland	42 24	76 00		T. R	5
Wakeley, Chas. C., Rutherford's Observatory.	New York	New York	40 44	73 59	41	A	1
Warren, James H	Suffern	Rockland	41 30	74 31		T. R	1
White, Aaron	Cazenovia	Madison	42 55	75 46	1,260	A	6
Willis, Oliver R	White Plains	Westchester	41 05	73 40		T	12
OHIO.							
Abell, B. F	Welshfield	Geauga	41 23	81 12	1,205	T. R	12
Adams, D. P	Marietta	Washington	39 25	81 31	630	T. R	1
Atkins, Rev. L. S	Saybrook	Ashtabula	41 52	81 01	650	T	2
Benner, Josiah F	New Lisbon	Columbiana	40 45	80 45	961	B. T. R	12
Clark, Wm. P	Medina	Medina	41 07	81 47	1,255	A	2
Colbrunn, Edward	Cleveland	Cuyahoga	41 30	81 40	665	T	12
Cotton, D. B., M. D	Portsmouth	Scioto	38 45	82 50	523	B. T. R	1
Crane, George W	Bethel	Clermont	39 00	84 00	555	T. R	12
Dille, Israel	Newark	Licking	40 07	82 21	825	T	8
Dole, J. G; Grifling, C. S. S	Austinburg	Ashtabula	41 54	80 52	816	B. T. R	12
Engelbrecht, Lud	Portsmouth	Scioto	38 42	82 36	537	B. T. R	9
Fraser, James	Little Hocking	Washington	39 25	81 00		N	2
Hammitt, John W	College Hill	Hamilton	39 19	84 26	800	T. R	12
Harper, George W	Cincinnati	Hamilton	39 06	84 27	*500	A	12
Haywood, Prof. John	Westerville	Franklin	40 04	83 00		A	8
	Kingston	Ross	39 29	83 00	692	A	3
Hill, F. G	Dallasburg	Warren	39 30	84 31	800	N	1
Huntington, George C	Kelley's Island	Erie	41 36	82 42	587	B. T. R	12
Hyde, Gustavus A; Hyde, Mrs	Cleveland	Cuyahoga	41 30	81 40	643	B. T. R	12
Ingram, John, M. D	Savannah	Ashland	41 12	82 31	1,098	A	7
Jerome, A. E	New Westfield	Hood	41 13	83 49	692	T. R	2
King, Mrs. Ardelia C	Madison	Lake	41 50	81 00	620	T. R	2
Larsh, Thomas J	Eaton	Preble	40 00	74 00	1,400	T	1
McClung, Charles L	Troy	Miami	40 03	84 06	1,103	B. T. R	5
McMillan, Smith B	East Fairfield	Columbiana	40 47	80 44	1,152	A	12
Mathews, Joseph McD	Hillsborough	Highland				A	6
Newton, Rev. Alfred	Norwalk	Huron	41 15	82 30		T	12
Peck, Wm. R., M. D	Bowling Green	Wood	41 15	83 40	700	B. T. R	12
Peirce, Warren	Garrettsville	Portage	41 15	81 10	900	T	3
Phillips, R. C. and J. H	Cincinnati	Hamilton	39 06	84 27	540	B. T. R	12
Samms, Dr. C. C	Hillsborough	Highland			1,120	T. R	2
Schauber, Hubert A	Cardington	Morrow	40 30	83 00		N	1
Smith, C. H., M. D	Kenton	Hardin	41 30	84 41		B. T	1
Thompson, Rev. David	Milnersville	Guernsey	40 10	81 45		T	12

* Above low-water in the Ohio river at Cincinnati.

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
OHIO—Continued.			° ′	° ′	Feet.		
Thompson, Rev. Elias	Croton	Licking	40 13	82 38		T. R	3
Thompson, Prof. H. A	Westerville	Franklin	40 04	83 00		A	4
Trembley, J. B., M. D	Toledo	Lucas	41 39	82 32	604	B. T. R	12
Ward, Rev. L. F	Wellington	Loraine	41 08	81 13	875	A	7
Warder, A. A	Cincinnati	Hamilton	39 08	84 35	800	T. R	6
Williams, Prof. M. G	Urbana	Champaign	40 06	83 43	1,015	B. T. R	12
Wilson, Prof. J. H	College Hill	Hamilton	39 19	84 25	800	B. T. R	12
Young, Prof. Chas. A; Elliott, J. C	Hudson	Summit	41 15	81 24	1,137	B. T. R	6
OREGON.							
Ironside, R. B	Auburn	Baker	44 37			T	1
Willis, P. L	Salem	Marion	44 56	123 01	120	B. T. R	1
PENNSYLVANIA.							
Atwater, H. H	Susquehanna Depot.	Susquehanna	42 00	75 30		T. R	2
Bentley, E. T	Tioga	Tioga				T	2
Boyers, W. R	Blairsville	Indiana	40 31	74 43	1,010	T. R	10
Bruckart, H. G	Silver Spring	Lancaster	40 05	76 45		T	9
Brugger, Samuel	Fleming	Centre	40 55	77 53	780	T. R	12
Darlington, Fenelon	Parkersville	Chester	39 54	75 37	218	T. R	7
Eggert, John	Berwick	Columbia	41 05	76 15	583	A	5
Friel, P	Shamokin	Northumberland	40 45	76 30	700	T. R	1
	Philadelphia	Philadelphia	39 57	75 50	50	T. R	1
Hance, Ebenezer	Morrisville	Bucks	40 12	74 48	30	B. T. R	12
Heisley, Dr. John	Harrisburg	Dauphin	40 16	76 15		A	12
Hickok, W. O	Harrisburg	Dauphin	40 20	76 50	320	A	12
Hoffer, Dr. Jacob R	Mount Joy	Lancaster	40 08	76 30		A	11
Jacobs, Rev. M; Jacobs, H. E	Gettysburg	Adams	39 49	77 15	624	B. T. R	7
Kirkpatrick, Prof. Jas. A	Philadelphia	Philadelphia	39 57	75 10	50	A	12
Kohler, Edward	North Whitehall	Lehigh	40 40	75 26	250	T	7
Lyceum, Jefferson College	Cannonsburg	Washington	40 17	80 10	936	A	4
Martindale, Isaac C	Byberry	Philadelphia	40 05	75 00	70	T. R	12
Meehan, Thomas; Meehan, J	Germantown	Philadelphia				T	11
Muller, Prof. Rudolph	Pittsburg	Alleghany	40 30	80 09	937	B. T. R	3
Ralston, Rev. J. Grier	Norristown	Montgomery	40 08	75 19	153	A	12
Ricksecker, Lucius E	Nazareth	Northampton	40 43	75 21	530	T	4
Savery, Thos. H	Altoona	Blair	40 35	78 22	1,178	T	4
Smith, Wm., D. D	Cannonsburg	Washington	40 16	80 10	936	B. T. R	12
Swift, Dr. Paul	West Haverford	Delaware	40 00	75 21	400	T. R	6
Taylor, John	Connellsville	Fayette	40 00	79 36		T	12
Walker, Robert L	Moorhead	Alleghany				T	4
Weeks, James A	Oil City					T	12
RHODE ISLAND.							
Caswell, Prof. Alexis	Providence	Providence	41 49	71 25	120	A	10
Sheldon, H. C	Providence	Providence	41 50	71 25		B. T. R	7
SOUTH CAROLINA.							
Marsh, M. M., M. D; Marsh, Mrs	Beaufort	Beaufort	32 20	80 46	1	B. T. P	5
TENNESSEE.							
Stewart, Prof. Wm. M	Clarksville	Montgomery	36 28	87 13	481	A	7
UTAH.							
Pearce, Harrison	St. George	Washington	37 00	114 00		T. R	3
Phelps, W. W	Salt Lake City	Salt Lake	40 45	111 26	4,260	A	2
VERMONT.							
Buckland, David	Brandon	Rutland	43 45	73 00		T. R	12
Chickering, Rev. J. W	Springfield	Windsor	43 18	72 33	300	T. R	4
Cutting, Hiram A	Lunenburg	Essex	44 28	71 41	1,124	A	12
Marsh, M. M., M. D	Montpelier	Washington	44 17	72 36	540	B. T	5

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
VERMONT—Continued.			° ′	° ′	*Feet.*		
Mead, Stephen O	Rutland	Rutland	....	....	....	T	5
Paddock, James A	Craftsbury	Orleans	44 40	72 29	1,100	T. R	12
Parker, Joseph	West Rupert	Bennington	43 15	73 11	750	T	3
Petty, McK	Burlington	Chittenden	44 27	73 10	367	A	12
Pollard, T. F	Brookfield	Orange	44 02	72 36	....	T. R	5
Toboy, James K	Calais	Washington	44 22	72 09	....	T. R	1
WASHINGTON.							
Swan, James G	Neeah Bay	....	28 41	124 37	40	T	12
WISCONSIN.							
Armstrong, S	Waterford	Racine	42 48	88 13	....	T	5
Curtis, W. W	Rocky Run	Columbia	43 26	89 20	....	T. R	8
Ellis, Edwin, M. D	Odanah	Ashland	46 33	91 00	610	T. R	9
Gridley, Rev. John	Kenosha	Kenosha	42 35	87 50	600	B. T. R	1
Kelley, Charles W	Delafield	Waukesha	43 06	88 36	900	B. T	6
Lapham, Incr'se A., LL.D	Milwaukee	Milwaukee	43 03	87 59	593	A	12
Lups, Jacob	Manitowoc	Manitowoc	44 07	87 45	658	B. T	12
Mann, William	Superior	Douglas	46 46	92 03	680	T. R	6
Mathews, George	Brighton	Kenosha	42 36	88 03	700	N	6
Mead, H. C	Waupaca	Waupaca	44 20	89 11	....	T	1
Porter, Henry D	Beloit	Rock	42 30	89 04	750	B. T. R	12
Sterling, Prof. John W	Madison	Dane	43 05	89 25	1,068	A	10
Whiting, Wm. H	Geneva	Walworth	42 30	89 41	....	T	8
Winkler, Carl, M. D	Milwaukee	Milwaukee	43 03	87 57	600	B. T. R	12
Woods, William	Weyauwega	Waupaca	44 15	88 50	850	T	12

DEATHS OF OBSERVERS.

Dr. S. P. Hildreth, Marietta, Ohio, July 24, 1863.
T. F. Pollard, Brookfield, New Hampshire, August 19, 1863.
Hon. Robert Hallowell Gardiner, Gardiner, Maine, March 22, 1864.
David Buckland, Brandon, Vermont, July 19, 1864.

Colleges and other institutions from which meteorological registers were received during the year 1863, *included in the preceding list.*

Nova Scotia	Acadia College	Wolfville.
	King's College	Windsor.
Canada	Grammar School	Niagara.
	Magnetic Observatory	Toronto.
Connecticut	Wesleyan University	Middletown.
Illinois	Lombard University	Galesburg.
	University of Chicago	Chicago.
Iowa	Cornell College	Mount Vernon.
	Griswold College	Davenport.
	Iowa State University	Iowa City.
Maine	Oak Grove Seminary	Vassalboro'.
Maryland	Washington College	Chestertown.
Massachusetts	Amherst College	Amherst.
	State Lunatic Hospital	Worcester.
	Williams College	Williamstown.
Michigan	State Agricultural College	Lansing.
New Jersey	Freehold Institute	Freehold.
New York	Institution for Deaf and Dumb	New York.
	Erasmus Hall Academy	Flatbush.
	University of Rochester	Rochester.

Colleges, &c., from which meteorological registers were received, &c.—Continued.

Ohio	Farmers' College	College Hill.
	Halcyon Academy	Croton.
	Otterbein University	Westerville.
	Urbana University	Urbana.
	Western Reserve College	Hudson.
	Woodward High School	Cincinnati.
Oregon	Willamette University	Salem.
Pennsylvania	Central High School	Philadelphia.
	Haverford College	West Haverford.
	Jefferson College	Cannonsburg.
Rhode Island	Brown University	Providence.
Tennessee	Stewart College	Clarksville.
Vermont	University of Vermont	Burlington.
Wisconsin	Beloit College	Beloit.
	Wisconsin University	Madison.

METEOROLOGICAL MATERIAL CONTRIBUTED IN ADDITION TO THE REGULAR OBSERVATIONS.

Abbott, Francis.—Abstract of observations made at eight stations in Tasmania, or Van Dieman's Land, during the six months ending June, 1862, for the papers and proceedings of the Royal Society.

Caswell, Prof. A., D. D.—Summary for the year 1863, and comparison with the previous thirty-two years, at Providence, Rhode Island. Printed in the Providence Daily Journal.

Dabney, William H.—Temperature of the valley of Orotava, Island of Teneriffe, compared with that of London, Paris, Pau, Nice, Rome, and Madeira. Extracted from the pamphlet of the Baron of Belcastel.

Dreutzer, O. E., (consul, Bergen, Norway.)—Summary of meteorological observations for each month in the year 1863, kept at the hospital in Bergen. The readings of the barometer reduced to inches, and the thermometer to Fahrenheit scale, by Mr. Dreutzer.

Gardiner, R. H.—Printed summary of observations during the year 1863, at Gardiner, Maine, and monthly mean temperature and amount of rain for a period of twenty-seven years, from 1837 to 1863, inclusive.

Goddard, C. W.—Daily observations at Constantinople, from October, 1862, to September, 1863, inclusive. Also a summary for the year 1862.

Gregory, S. O.—Diagram showing the changes of the wind every day in the year 1863, at Theresa, New York.

Graham, Colonel James D.—Register of water-level and meteorological observations, made at the following places, under the direction of Captain George G. Meade, topographical engineers, until August, 1863, and subsequently under the direction of Colonel James D. Graham, corps of engineers, superintendent of the survey:

Sackett's Harbor, New York.—July, 1861, to December, 1863.
Charlotte, New York.—July, 1861, to December, 1863.
Fort Niagara, New York.—July, 1861, to December, 1863.
Buffalo, New York.—June, 1860, to December, 1863.
Cleveland, Ohio.—June, 1860, to December, 1863.
Monroe, Michigan.—July, 1861, to December, 1863.
Detroit, Michigan.—January, 1860, to December, 1863.
Tawas City, Michigan.—July, 1861, to December, 1863.
Thunder Bay Island, Michigan.—July, 1861, to November, 1863.
Sugar Island, Michigan.—November, 1863, to December, 1863.
Grand Haven, Michigan.—July, 1861, to July, 1863.
Ontonagon, Michigan.—July, 1861, to December, 1863.
Superior, Wisconsin.—June, 1861, to December, 1863.

Ives, William.—Summary of observations at Buffalo, New York, during the year 1863, newspaper slip.

Kirkpatrick, Professor James A.—A general abstract of the meteorological observations made at Philadelphia during the year 1863, and a comparison with those of the last twelve years. Printed sheets from the Journal of the Franklin Institute.

Lake Winnipisseogee Cotton and Woollen Manufacturing Company, New Hampshire.—Amount of rain for each month in 1863, at the outlet of Lake Winnipisseogee, in the town of Laconia, New Hampshire, and also at Lake Village, about four miles south on the same stream of water.

Lapham, I. A., LL.D.—Table showing the direction and force of the wind for each hour during the month of September, 1863, at Milwaukee, Wisconsin, taken from the autographic record made by Burnell's anemograph. Prepared for the Commissioner of Agriculture by I. A. Lapham, LL.D.

Summary of observations at Milwaukee, Wisconsin, with a full set of instruments, during the year 1863. (Printed slip from the Milwaukee Sentinel.)

Lewis, James, M. D.—Hourly record of the temperature at Mohawk, New York, during the year 1863, from the register made by his metallic self-recording thermometer; also, monthly and half-monthly means, and hourly mean for the whole year 1863.

Lippincott, James S.—Meteorological observations made by Benjamin Shepherd near Greenwich, Cumberland, New Jersey, from March, 1856, to June, 1861. Tabulated and reduced by James S. Lippincott, Haddonfield, Camden county, New Jersey, for the Smithsonian Institution.

Logan, Thomas M., M. D.—Monthly summaries of the meteorology and necrology of Sacramento, California, reported for the Sacramento Daily Union by Thomas M. Logan, M. D., secretary of the Board of Health.

Contribution to the Physics, Hygiene, and Thermology of the Sacramento River, by Thomas M. Logan, M. D. From the Pacific Medical and Surgical Journal. 8 pp. 8vo.

Magnetical Observatory, Toronto, Canada West, (Professor G. T. Kingston, M. A., director.)—Mean meteorological results for the year 1862; also, a comparison of the same with a series of preceding years.

Mayhew, Royal.—Mean temperature at Indianapolis, Indiana, for the hours of sunrise, 7 a. m., 12 m., and 2, 6, and 9 p. m., during each month in the years 1861, 1862, and 1863; also, the amount of rain in each month during the same period.

Morris, Prof. Oran W.—Summary of observations for 1863, giving maximum, minimum, mean, and range of all the instruments for each month, as kept at the Institution for the Deaf and Dumb, New York.

Murdock, G.—Appendix to Agricultural Report, being hints on meteorology, with summaries of observations made at Saint John, New Brunswick, in the years 1851 to 1862, inclusive, by G. Murdock, superintendent of water-works at St. John. 8vo. 34 pages.

Nason, Rev. Elias.—Record of events in Exeter, New Hampshire, during the year 1863, containing notices of the weather. No. 3, by the Rev. Elias Nason. 12mo. 24 pages.

Ohio State Board of Agriculture.—Fifteenth Annual Report of the board to the general assembly of Ohio for the year 1860. Contains articles on the influence of forests upon soil, climate, rain, and winds. P. 255 to 274.

Report for 1861. "The atmospheric conditions, showing the value of barometers for agricultural purposes," by C. A. Richard, of Columbus, Ohio. P. 234 to 275.

Osservatorio del Collegio Romano.—Bulletino Meteorologico del' Osservatorio del Collegio Romano con corrispondenza e bibliografia per l' avanzamento della fisica terrestre. Published at Rome twice a month, beginning March, 1862.

Paine, H. M., M. D—Summary of observations at Clinton, New York, for 1862 and 1863, with a full set of instruments, giving the monthly and annual means, maxima, and minima.

Paterson, Rev. A. B—Meteorological notes for December, 1863, at St. Paul, Minnesota, with a comparison with the previous four years. (Newspaper.)

Riotte, C. N.—Printed summary of observations made at seven stations in Costa Rica in the year 1863.

Sartorius, Charles.—Summary for the year 1863, with full set of instruments, at Mirador, Mexico.

Secchi, P. Angelo.—Alcune richerche meteorogiche sulle tempeste occorse nel 1859–'60 memoria del P. Angelo Secchi. Estratta dagli Atti della accademi de' Nuovi Lincei Sessione III, dell' Anno XIII, del 5 febbraro 1860. Rome, 1860. 28 pp. quarto.

State Department.—Statistical report on the weather and health of Frankfort-on-the-Main during the year 1863, by William W. Murphy, consul.

Vaughan, Captain D.—Meteorological Journal and Report relative to the currents, climate, and navigation of that portion of the lower St. Lawrence forming the Strait of Belle-Isle. Second edition. Compiled by Captain D. Vaughan, Quebec. 8vo. 62 pp.

Whitehead, W. A.—Summary of observations during the year 1863 at Newark, New Jersey. Printed slip from Newark Daily Advertiser. Also, an article on the "Climate of Newark," being an examination and comparison of the observations made there during the last twenty years.

Wislizenus, A., M. D.—Monthly and yearly mean of positive atmospheric electricity, of temperature, and of relative humidity, in 1861, 1862, and 1863, at St. Louis, Missouri, based upon daily observations at 6, 9, 12, 3, 6, and 9 o'clock. Published in the St. Louis Medical and Surgical Journal. Vol. I, No. 1.

REPORT OF THE EXECUTIVE COMMITTEE.

The Executive Committee respectfully submit to the Board of Regents the following report of the receipts and expenditures of the Smithsonian Institution during the year 1863, with estimates for the year 1864:

General Statement.

RECEIPTS.

The whole amount of the Smithson bequest deposited in the treasury of the United States is $515,169, from which an annual income at 6 per cent. is derived of	$30,910 14
The extra fund of unexpended income is invested as follows, viz:	
In $75,000 Indiana 5 per cent. bonds, yielding (less United States tax)	3,749 50
In $53,500 Virginia 6 per cent. bonds.	
In $12,000 Tennessee 6 per cent. bonds.	
In $500 Georgia 6 per cent. bonds.	
In $100 Washington city 6 per bonds, yielding	6 00
Total income	34,665 64
Balance in the hands of the treasurer, January, 1863	29,509 61
Total receipts	64,175 25

EXPENDITURES.

For building, furniture, and fixtures	$2,111 78	
For general expenses	11,688 69	
For publications, researches, and lectures	10,761 65	
For library, museum, and gallery of art	7,259 23	
		31,821 35
Balance in the hands of the treasurer, January, 1864		32,353 90

STATEMENT IN DETAIL OF THE EXPENDITURES OF 1863.

Building incidentals	$1,598 79	
Furniture and fixtures	512 99	
		$2,111 78
Meetings of the Board of Regents	104 50	
Lighting	343 71	
Heating	1,090 75	
Postage	421 46	
Transportation, general	374 05	
Exchanges	1,357 76	
Stationery	486 09	

General printing	$3 50	
Apparatus	531 98	
Laboratory	129 59	
Incidentals, general	584 65	
Extra clerk-hire	371 65	
Salaries, secretary	3,500 00	
Salaries, chief clerk, bookkeeper, laborers, &c	2,389 00	
		$11,688 69
Smithsonian contributions	2,545 48	
Smithsonian reports	583 85	
Smithsonian miscellaneous collections	3,535 88	
Smithsonian and other publications	441 15	
Meteorology	2,410 97	
Researches and investigations	150 00	
Lectures	1,094 32	
		10,761 65
Cost of books and binding	1,844 65	
Pay of assistants in library	1,100 00	
Transportation for library	290 35	
Incidentals for library	24 15	
Museum, salary of assistant secretary	2,000 00	
Transportation for museum	695 29	
Incidentals for museum	395 40	
Explorations for museum	762 39	
Gallery of art	147 00	
		7,259 23
		$31,821 35

The whole income during the year 1863 was $34,665 64, corresponding with the estimate in the report for 1862. The expenditures during the year 1863 were $31,821 31, leaving $2,844 33 to be added to the balance in the hands of the treasurer at the beginning of the year.

The amount of bills outstanding will not exceed $2,000.

The foregoing statement is an actual exhibit of the Smithsonian funds irrespective of credits and payments made in behalf of other parties. The Institution has during the year paid several bills for work done and articles purchased on account of the government, part of which has been refunded and credited to the appropriation from which the expenditure was originally made. Those which have been refunded are as follows: $476 87 from the Surgeon General's office for books purchased in Europe through the agency of the Institution; and $37 from the Naval Observatory for transportation. In addition to these, several expenditures have been made on account of the Light-house Board for photometrical apparatus, and experiments in the laboratory, which have not yet been refunded.

Messrs. Rice & Kendall, of Boston, have also refunded $93 80 for paper purchased of them remaining in their hands not used.

The appropriations from Congress for the preservation of the collections and the distribution of the duplicate specimens of the exploring and surveying expeditions of the government have been expended, as heretofore, under the direction of the Secretary of the Interior in assisting to pay the expenses of assistants in the museum, and the cost of arranging, labelling, and preserving the specimens. The sums thus received have been credited to the museum, and have served to diminish the apparent amount of expenditures for that object.

The estimated expenditures for 1863 were as follows:

For building, furniture, and fixtures	$2,000
For general expenses	10,500
For publications, researches, and lectures	10,500
For library, museum, and gallery of art	9,000
Total	$32,000

The actual expenditure on the building is very nearly the same as the amount appropriated.

For general expenses the amount is larger than the estimate, and this is due to the increased cost of materials.

For publications, &c., the actual expenditure is nearly the same as the estimate.

For library, museum, and gallery of art, the expenditure is nearly three thousand dollars less than the estimate, but this is on account of the expenditure on the collections of the remainder of an appropriation from Congress for the distribution of the specimens.

For the year 1864 the same estimates are recommended as those made for 1863.

The committee have examined the books and accounts of the Institution for the past year, and find them to be correct.

Respectfully submitted.

A. D. BACHE,
RICHARD WALLACH,
Committee.

JOURNAL OF PROCEEDINGS

OF

THE BOARD OF REGENTS

OF

THE SMITHSONIAN INSTITUTION.

WASHINGTON, *January* 20, 1864.

In accordance with a resolution of the Board of Regents of the Smithsonian Institution, fixing the time of the beginning of their annual session on the third Wednesday of January of each year, the Board met this day in the Regents' room, at 10½ o'clock a. m. Present: Hon. S. S. Cox, Hon. J. W. Patterson Hon. R. Wallach, General J. G. Totten, and Professor Henry, the Secretary.

A quorum not being present, the Board adjourned to meet on Monday, January 25, at 7½ p. m.

MONDAY, *January* 25, 1864.

A meeting of the Board of Regents was held this day at 7½ o'clock p. m Present: Hon. H. Hamlin, Vice-President of the United States, Hon. W. P. Fessenden, Hon. L. Trumbull, Hon. J. W. Patterson, Hon. H. W. Davis, Hon. R. Wallach, Mr. William B. Astor, General Joseph G. Totten, Professor A. D. Bache, the treasurer Mr. Seaton, and Professor Henry, the Secretary.

In the absence of the chancellor, Mr. Hamlin was called to the chair.

The Secretary announced the election, by joint resolution of the Senate and House of Representatives, of Professor Agassiz, of Massachusetts, as a Regent in place of Mr. Badger, the reappointment by the Speaker of Hon. S. S. Cox, of Ohio, and the appointment of Hon. J. W. Patterson, of New Hampshire, and Hon. Henry Winter Davis, of Maryland, as Regents from the House of Representatives.

The general statement of the funds of the Institution and of the receipts and expenditures during 1863 was presented by the treasurer.

The Secretary submitted the annual report of the operations of the Institution during the past year, which was read in part.

The Secretary made a statement as to the policy which had been adopted in regard to bequests and donations having special conditions attached to them,

and gave the reasons for declining to accept a herbarium which had recently been bequeathed to the Institution.

On motion it was

Resolved, That the action of the Secretary in this case be approved.

The Secretary called attention to the unexpected delays and embarrassments which had occurred in obtaining the remainder of the original bequest of Smithson left in England as the principal of an annuity to the mother of the nephew of Smithson, and read the correspondence on the subject with the attorneys, and also a letter from Hon. C. F. Adams, the American minister to England.

On motion it was

Resolved, That a committee be appointed, consisting of the Secretary, Mr. H. W. Davis, and Professor Bache, to confer with the Secretary of State and the British minister relative to the action of the English authorities in regard to the money due the Smithsonian Institution.

On motion, the Board adjourned to meet on Wednesday, January 27, at 7½ o'clock p. m.

WEDNESDAY, *January* 27, 1864.

A meeting of the Board of Regents was held at the Institution at 7½ o'clock p. m. Present: Hon. H. Hamlin, Vice-President of the United States, Hon. G. Davis, Hon. R. Wallach, Mr. William B. Astor, Professor A. D. Bache, and the Secretary.

Mr. Hamlin was called to the chair.

The minutes of the last meeting were read and approved.

Professor Bache presented the report of the executive committee, which was read and approved.

The Secretary presented the remainder of his annual report, which was read and adopted.

He also presented a series of letters illustrating the correspondence and operations of the Institution.*

On motion, the Board adjourned to meet at the call of the Secretary.

TUESDAY, *March* 15, 1864.

A meeting of the Board of Regents was held this day at 10½ o'clock a. m. Present: Hon. H. Hamlin, Vice-President of the United States, Hon. S. S. Cox, Hon. J. W. Patterson, Hon. R. Wallach, Professor L. Agassiz, Professor A. B. Bache, and the Secretary.

Mr. Hamlin was called to the chair.

The minutes of the last meeting were read and approved.

The Secretary presented a series of works on natural history, which had been prepared and printed at the expense of the Institution, and also the

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* See end of the Proceedings, page 80.

manuscripts of several others which had been offered for publication. All of these, he stated, had been referred for critical examination to Professor Agassiz, who would favor the Board with some remarks in regard to them.

Professor Agassiz stated that, so far as he had had an opportunity of examining the original papers, he considered them worthy of publication; that he would give the whole series of works on natural history, which constitute portions of what is called the Miscellaneous Collections, a critical examination, and present a report upon them at a future time. At present he would beg leave to make a few remarks on the importance of adopting measures for increasing the efficiency of the active operations of the Institution by relieving them of the expense of the support of the museum, library, and gallery of art. Unless this could be done, many valuable contributions to science offered for publication would have to be postponed or refused. He thought that the resources of the Institution were inadequate to carry on at the same time its active operations, and maintain a museum, a library, and a gallery of art upon the only footing upon which they can truly be creditably supported. Without, therefore, making a definite motion, he would submit for future consideration the propriety of asking the government to take charge of the museum, the library, and the building now occupied by the Institution, with a view of maintaining them on a more extensive scale, and relieving the Smithsonian Institution of a large expenditure which, for the advancement and diffusion of science, had better hereafter be devoted to the active operations of the Institution. He hoped that if such a plan would be carried out, the resources reverting to the Institution from the transfer of the museum and library to the government, either to form an independent organization or to be carried on hereafter as before by the Smithsonian Institution, the active operations of the latter would be greatly extended.

The Secretary stated that the suggestions of Professor Agassiz were in accordance with the views which had been entertained by the majority of the Board of Regents from the first discussion of the organization of the Institution; that the present disposition of the funds was a necessity which was imposed upon the directors by the requirements of the law of Congress establishing the Institution, and that he had always entertained the hope that the support of the building and collections would in due time be provided for by the general government, and a national museum be founded which would be commensurate with the intelligence, extent, and resources of the country.

Professor Bache fully concurred in these remarks, and moved the following resolutions, which were adopted:

Resolved, That a committee be appointed to report to the Board of Regents any suggestions for extending the active operations of the Smithsonian Institution, and for the separate maintenance of the collections.

Resolved, That this committee consist of Professor Agassiz, the Secretary of the Institution, Mr. Fessenden, Mr. Patterson, and Mr. Cox.

The Board then adjourned *sine die.*

LETTERS PRESENTED TO THE BOARD OF REGENTS TO ILLUSTRATE THE CORRESPONDENCE AND OPERATIONS OF THE INSTITUTION.

Communication from Dr. B. A. Gould, on a new discussion and reduction of the observations of Piazzi of Palermo.

CAMBRIDGE, *May* 16, 1863.

MY DEAR SIR: For many years I have been strongly convinced that an extremely valuable contribution to astronomical science might be made by a new discussion and reduction of the observations of Piazzi at Palermo.

This eminent astronomer, with his assistants, was engaged, during the twenty-two years from 1792 to 1813, in observing the positions of the principal fixed stars. He was provided with the best instruments which could be obtained at that time, and his observations have been, and must continue to be, our principal and most trustworthy source of information as to the places of between seven and eight thousand fixed stars at the beginning of the present century. As nearly as I can estimate without an actual count, he must have made about ninety thousand determinations of right ascension, and from sixty to seventy thousand of declination, the original records of which observations still exist. From these he constructed his two well-known catalogues—the first in 1803, the second in 1814—containing the mean places for 1800.0 of 7,646 stars.

His methods of observation, while, of course, far inferior in many respects to those of the present day, were the best in use at that period; and the care and fidelity with which they were used seem to have been unsurpassed; and, although the reductions upon which the catalogue was based seem to have been incommensurate in precision with the observations themselves, still this catalogue has, for the past fifty years, been a standard authority with astronomers, and, for a great part of that time, their chief dependence for both the right ascensions and declinations of stars.

The original observations of Piazzi were sent by him for safe keeping to his friend Oriani, in Milan, and have been carefully preserved at the Observatory of the Brera in that city. In 1845, Professor Littrou, the director of the Imperial Observatory of Vienna, incited specially, as he says, by Argelander, and encouraged by Bessel, Gauss, Schumacher, Struve, &c., commenced the printing of these original observations as part of the series of Annals of the Vienna Observatory, and they have thus been for several years accessible to astronomers.

When organizing the Dudley Observatory in 1856–'58, it formed an integral part of my plan, not merely to institute new observations of the heavenly bodies, but to carry on such computations, reductions, &c., as might render available past observations of this and the last century, which would otherwise be either useless or of inferior value to astronomy. Various undertakings of this kind were planned, but the first of all to be begun was the re-reduction of the whole series of Piazzi's observations, using the best values of the constants of precession, aberration, and mutation, and investigating all the instrumental errors with care; and I made considerable progress in arranging the details of the computation. After communication with Professor Littrou, and an extended correspondence with Professor Argelander on the subject, in which this distinguished astronomer gave me many very useful suggestions, the whole plan was completed, and, but for the misfortunes which interfered with the usefulness of the Dudley Observatory before its activity had fairly begun, the new catalogue would doubtless now have been in the hands of astronomers.

My health and opportunities of labor being now greatly improved, I am anxious to resume this work, and write to ask for your influence and aid, as far as possible, in furtherance of the plan. Knowing, as you do, the nature of the

work proposed, it is almost needless to dwell upon its value to science. The one consideration, that Piazzi's observations must, for long years to come, furnish the only means of determining the proper motions of more than five thousand stars, is of itself sufficient. For the other stars observed by him, they constitute a most important element in the determination. The huge number of stars, observed in zones by Lalande, at almost the same period—more than fifty thousand—depend for their reduction and value almost solely upon Piazzi's results; and the formation of a new catalogue of the latter will give an altogether new value to the results of Lalande. The great mass of independent observations thus rendered more accurate can speak for themselves, and it is manifest that their usefulness will be far greater than that of the same number of new observations made now.

Unfortunately, Piazzi's observations do not afford all the elements now known to be needed for their reduction, and it will doubtless be necessary to reduce them differentially, thus greatly increasing the labor. Not merely questions of azimuth, zenith point, and clock correction, but also questions of graduation, of irregularity of pivots, and even of refraction, must be discussed, thus rendering the undertaking one of no small magnitude; still it would, I am sure, be labor well bestowed, and, as Professor Argelander wrote me in 1857, "it would be a grand thing, * * * * and one of the most important things that could be done."

The first process required is the reduction to the mean equinox of 1800.0 of all the observations just as they were given by Piazzi. This is a work which could be carried on by ordinary computers, and would in itself be of great service, even were the discussions of the observations to be omitted. It would constitute nearly two-thirds of all the labor, and possesses the great advantage that whatever is done, be the amount large or small, is immediately available. The best estimate that I am able to make gives about $5,000 as the probable cost of this reduction, to which from one-quarter to one-third should be added for the expense of checking, comparing, and correcting mistakes. Therefore, before beginning, I desire to make sure that at least $6,000 will be available for the purpose. There is scarcely a limit to the number of computers who could be employed at once upon this part of the work. It might easily be accomplished in a single year, or it might be slowly and regularly carried on for a long time, the expense being not very different in the two cases.

This process being completed, the remainder of the work, consisting of various investigations, in addition to the discussion of the instrumental corrections, and the formation of a catalogue from the observations after all reductions have been applied, would, of course, require more deliberate study. It would probably occupy at least two years, but I think the expense would be decidedly inferior to that of the first process. Indeed, I have convinced myself that all the outlays needed for the whole undertaking in all its branches would not exceed $10,000, and that if this sum were now available, the work might be completed in two years, inasmuch as parts of all the processes could go on simultaneously.

My sense of the usefulness of this work, and my conviction that astronomers everywhere would agree in this opinion, are so strong that I have determined to appeal to you for aid, well knowing that your interest and moral support will, under any circumstances, not be wanting. It is precisely such an undertaking as the plan of the Smithsonian Institution would lead it to encourage; and although I can readily see that the amount needed is larger than the Smithsonian would probably be able to apply at any one time to the furtherance of any one science, still I come to you with my plan, well assured that you will willingly do what you can in its behalf, whether by some gradual appropriation year after year, from the Smithsonian funds, in aid of what I have called the first process, viz.: The computation of the correction to the mean equinox of

1800.0, or in some still more active way, by enlisting interest and securing aid from other sources.

For several months past I have devoted such time and means as I could to the preliminary steps, and, as you are aware, I now desire only the means of defraying the indispensable outlays, wishing to contribute my own services in behalf of the work.

I am, dear sir, very respectfully and truly yours,

B. A. GOULD.

Professor JOSEPH HENRY,
Secretary of the Smithsonian Institution.

Project of an outline history of public education in the United States, by Frederic A. Packard.

The proposed volume to contain from 600 to 800 pages royal 8vo, to be put up in a cheap form, in the manner of legislative documents, with ample tables, indexes, &c., for easy reference. If it shall be thought best, the form might be changed to *two* volumes—one embracing the original thirteen States, and the other the remaining States and Territories. The plan of the work would comprise the following topics:

I. *Of universal education,* considered as an essential element of free political institutions, what should be its character and extent?

II. An historical sketch of the laws of the several States on the subject of education, and the establishment of public schools, academies, and colleges. In this connexion would be given the provisions for education under the colonial government, and their influence on succeeding legislation.

III. An abstract or synopsis of all laws now in force in the several States touching public education, and of contemporaneous judicial expositions of the law, so far as they affect the essential principles of the system.

IV. A sketch of the present state of public education in the country:

(a.) Of the *division of territory* for school purposes, what and how made?

(b.) Of the manner of *raising money* for the support of schools, and the amount raised and expended in each decade of years, of the present century.

(c.) Of the *permanent revenue* for the support of schools—if derived from a fund—when and how was such fund created, and what is its amount and investment? what portion of the annual school expense is derived from it, and what is its effect to stimulate or depress the working of the system?

(d.) Of the number and average age of *children* under instruction, distinguishing the sex; the number in attendance, in proportion to the whole population, and the average time of attendance.

(e.) Of the mode of employing *teachers* and determining their qualifications.

(f.) Of the *number* of teachers employed, distinguishing the sex; the compensation allowed; the average age of teachers, male and female separate; and the average amount of time employed in daily teaching, making distinct heads of summer and winter schools.

(g.) Of the *branches* taught in the public schools, and the proportion of time devoted to each.

(h.) Of the preparation and introduction of *school-books;* character of them in early schools—improvements in them; expense of them, and by whom borne; and the number and variety of them, in the different branches, which are in use in the different schools.

V. Of *normal schools,* number, when organized, how supported, number of pupils, terms and condition of admission; what proportion of pupils pursue teaching for a livelihood, and what proportion of these succeed.

VI. Of *school-houses*, their number, average capacity, manner and means of building, and improvements in respect to site, ventilation, heating, furniture, out-houses, &c., &c.

VII. Of *school libraries*, number of schools supplied with; how and by whom selected; funds to purchase, and the amount and source of the same; number and character of volumes; cost, mode of distributing, preserving, and extent of circulation.

VIII. Of the *religious element* in public schools; if less than formerly, why? To what extent necessary and practicable?

IX. Of *popular manners and customs* in the schools; habits of thinking and acting; domestic and social character, and qualifications for citizenship, as they are influenced by our systems of public education.

X. Of *physical education*, what time appropriated to it; what facilities and encouragements are afforded; what methods adopted, as drill, gymnasium, or athletic games; and what part teachers take therein.

XI. Of *infant schools*.

XII. Of *Sunday schools*.

XIII. Of *colleges and other public literary institutions*, so far as they afford aid to, or receive aid from, the public schools.

XIV. Of the comparative *expense* and *value* of public education at different periods of our history.

XV. Of *lyceums, mechanics' institutes, evening schools*, and other methods of adult education, to make other means of education available, or to compensate for the want or neglect of early advantages.

XVI. *Number of persons of school age that are under instruction*, the proportion of the population that can both read and write; the qualifications of the pupils, upon leaving school, to engage in the active pursuits of life, with a superior *physical, moral*, and *intellectual* character.

The materials being thus collected, would be arranged under the title of each State, respectively, whatever is peculiar in its educational history and statistics being placed under specific heads, and what is common to all under general heads.

For example, *Maine* might occupy the first chapter or section of the volume—and we should first refer to Massachusetts for all matter preceding 1820, when it ceased to be a province. Then would come a succinct account of all legislation on the subject, including an abstract of existing laws; then the origin, amount, and mode of distributing any school fund. Next, a bird's-eye view of the actual condition of the schools, government, discipline, construction of buildings, character of teachers, text-books, and the obvious fruits of the system. Whatever peculiarity there may be in the climate, in the habits and pursuits of the people, or in the condition of society, affecting favorably or otherwise the interests of education, would find a place in this connexion.

After completing the circle of States in this way, a condensed chronological, historical, and statistical survey of the entire country would be in place, and such principles or conclusions as are established by the facts stated and illustrated.

It will be observed that the plan contemplates the history of each State *complete in itself*, and if prepared by an individual selected for the purpose, might bear the author's name, like contributions to a biographical dictionary or an encyclopædia. Of course it would serve a valuable *local* purpose, and if properly prepared, would secure a share of public patronage, while the *whole* volume would furnish highly interesting and important information to the country at large and to foreign inquirers.

When the outline thus sketched is well digested and matured, my purpose would be to forward a schedule of the subjects to some qualified patriotic person in each State, requesting his co-operation. The great advantages of having the

work done by a resident of the States, respectively, are the accuracy, fidelity, and fulness which would be secured, the facilities for obtaining materials, and the authority which it would bear. These considerations might induce one or more suitable persons in each State to encounter some personal inconvenience, especially as the service is one of vast and permanent importance, and can be better done now than at any future period.

The President of the Chamber of Commerce of Bordeaux to the Secretary of the Smithsonian Institution at Washington:

SIR: I am not ignorant that the Institution of which you are the Secretary, and which labors with the most praiseworthy zeal to promote the progress of the different branches of human knowledge, maintains relations of exchange with the Imperial Academy of Sciences, Belles Lettres, and Arts of Bordeaux.

The Chamber of Commerce, anxious in its turn to co-operate, as far as possible, in the realization of the plans which you pursue, feels pleasure in transmitting to you a copy of its publications. They comprise a collection of its proceedings since 1850, the first volume of the catalogue of its library, &c. It is hoped that these various publications will find a place in your collections. The Chamber has, on its own part, founded a considerable library, which is open to the public, and it would be happy if the Smithsonian Institution should think proper to send us some of the volumes which it publishes, and which are filled with documents of the greatest interest on America, and on different questions of importance. These works would thus be at the disposal of a considerable number of studious persons, and they would contribute to make the services of the Institution of which you are the organ appreciated in all their extent in Europe. Be pleased, sir, to accept the assurance of my most distinguished consideration.

CARTE DEL PALASIO, MILAN,
October 31, 1862.

SIR: Through the kindness of your agent, Mr. Bossange, of Paris, we have received the Annual Report of the Board of Regents, presented by the great and liberal Smithsonian Institution to the Carte del Palasio's Agricultural Association, of which we are directors and regents. Reading your valuable report, we have seen with the greatest satisfaction that the interesting and useful results of your labors have been approved and commended by intelligent men everywhere. Whilst expressing, honored sir, our warmest thanks for having been deemed worthy by your Institution to participate in the gifts which the liberality of the Smithsonian Institution renders to men devoted to science, it will be a source of pleasure to us to endeavor to reciprocate your kindness. To promote knowledge and facilitate its progress by stimulating men of science to undertake general and extensive researches, and to offer the means of continuing them, is the most useful service which can be rendered to mankind. The very extensive means which your great Institution has at its command, the ardor with which your officers and regents began and continue their difficult work, are infallible indications of the greatest results which will be produced. And we do not doubt that the material and moral progress of individuals, with that of science in general, will fully realize the anticipations of the founder, and amply recompense the continued labors of the distinguished directors of the Smithsonian Institution.

As directors of a new institution, which we hope will also soon produce important results in agriculture, we shall be content if, in reciprocating your kindness, we can also in any way serve the laudable purposes of your Institution by presenting the results of our own labors and researches.

Again expressing our thanks, we have the pleasure of sending some of the publications relating to our institution, with the hope that they will be placed in the Smithsonian library. They are the following: 1. Programme of organization of the Carte del Palasio's Agricultural Association. 2. Annual Reports of the Association for 1859–'61. 3. Agricultural Annals, by Dr. Gaetano Cantoni, professor of agronomy.

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Your most obedient servants,

SIG. ANTONIO RESCHIN, *Direttore.*
DR. GAETANO CANTONI, *Professor.*

OFFICE SUP'T U. S. MILITARY GENERAL HOSPITALS,
Memphis, Tennessee, September 5, 1863.

MY DEAR SIR: I am in receipt of your letter of the 25th ultimo, by which I learn the pleasing intelligence that the "great Tucson meteorite" is in a fair way of getting to Washington at last. I am sure you will feel proud of it when you see it. I knew the "Carlton specimen" was not ours, as I had sent it to Hermosilla before I left Arizona. That sent in by General C. is about 750 pounds, while ours is about twice that weight.

The only history I can give you is a vague one, as there is no written record of its advent in Tucson. The old inhabitants of that place all agree that it was brought there from the Santa Catarina mountains, which lie to the north of Tucson, about midway between the Rio San Pedro and that town. It was brought in by the military stationed at the old *presidio*, where it remained until after the withdrawal of the Spanish garrison. It was then taken into town, set up on end, and used as a kind of public anvil for the use of the inhabitants. The smaller one was used in a blacksmith's forge for similar purposes. In 1857 I found the large one lying in one of the by-streets half buried in the earth, having evidently been there a considerable time. No person claimed it, so I publicly announced that I would take possession of it in behalf of the Smithsonian, and forward it whenever an opportunity offered. Mr. Palatine Robinson, near whose house the iron was, assisted me in getting it sent to Hermosilla. There was some expense attending its hoisting into the truck-wagon that took it down to Sonora, which I paid to Mr. R. Mr. Ainsa agreed to take it, or have it taken, to Guaymas, Sonora, for fifty dollars.

The people of Tucson all agree that a shower of these meteorites fell in the Santa Catarina mountains some two hundred years ago, and I have been told that there were plenty of them remaining in the mountains. I never was in the immediate portion of the mountain range where they report the specimens are to be found, so I cannot vouch for the correctness of their reports. As the country is volcanic almost entirely, I have often thought, from the fact that iron ore is abundant in several of these mountains, that it might have been that masses of iron mineral were reduced to the metallic state by volcanic heat. See in the case of the famous "Planchas de plata" silver mines, some one hundred miles south of the Santa Catarina, where large pieces of pure silver have been found reduced to the pure state by fire, which has left everything in its vicinity in a state of calcination. One piece weighing 1,500 pounds was found and cut in two to allow its removal to the city of Mexico by the Spanish authorities. I think you will find allusion to those interesting and once rich mines in Brantz Mayer.

I believe I have given you some data about the Tucson meteorites in a monogram published by the War Department in 1860; Medical Statistics of United States Army, 1855–'60.

I wish I could give you full information on this matter. Please let me know when you receive it, and be assured that when I go to Washington I will pay my respects in person to you and it.

I am very busy, so you will excuse this hurried letter, and believe me

Yours, very respectfully,

B. J. D. IRWIN,
Surgeon United States Army.

SAN FRANCISCO, CAL., *July* 2, 1863.

DEAR SIR: The aerolite which had remained so long at Alamito, for want of a proper person to bring it here, was brought by one of my brothers, Jesus M. Ainsa, who visited Sonora lately. We have been induced to retain it here for a short time, to satisfy the curiosity of the San Francisco people. The State Geological Society asked to be allowed to have a small piece for their collection, which request was, of course, granted. With this exception the aerolite has been preserved entirely in the same condition in which it was found in Arizona, and by the 13th of this month we will have the pleasure to ship it to New York, under the care of the Pacific Mail Steamship Company.

I take this opportunity to offer my services to the Institution.

I remain, respectfully,

SANTIAGO AINSA.

Professor HENRY,
Smithsonian Institution, Washington, D. C.

SAN FRANCISCO, CAL., *August* 26, 1863.

DEAR SIR: I have the pleasure to acknowledge your favor of July 31, and I take pleasure in complying with your request. In fact I intended to do this before, but, owing to many engagements on hand, I have been postponing it to this moment.

I announced in my last that the meteorite would be sent by the following steamer from that date; but we were asked to retain it some time longer by some scientific men, who wished to examine it closely.

The history of this aerolite we have from our grandmother, Doña Ana Anza de Islas, daughter of Don Juan Bautista Anza, our great grandfather. The Jesuit missionaries had the earliest knowledge of this curiosity. There were various theories entertained about it; but it was generally believed to proceed from some iron mine in the vicinity, which belief holds to this day in Sonora. In an expedition made by Don Juan Bautista Anza, then "Gran Capitan de las Provincias del Occidente," about the year 1735, to the country about Tucson, he was induced to visit the aerolite, and he undertook the work of transporting it to Spain. The place where it was found is called "Sierra de la Madera," on a spot called Los Muchadios. Through the want of proper means and the bad state of the roads, (having to carry it to San Blas, then the nearest port of entry,) the work of transportation was given up, and they were satisfied to take it as far as Tucson. There it remained ever since, until my brother, Agustine Ainsa, undertook to transport it, in 1860, and present it to the Institute. His intentions, however, were never carried out until May last,

when another of my brothers, Jesus M. Ainsa, visited Sonora and brought it with him on his return.

By the time of the receipt of this the aerolite must be already in Washington, as we delivered it to the agent of the Institute about a month ago, to have it transported to you. Your agent spoke to us about expenses; but we wish not to deprive ourselves of the honor of having presented it to the Institute, and as such we desire that you should accept it.

I would be thankful if you would send me a copy of the analysis, and of other information about the aerolite; and if you find it not too troublesome, to send the same, with my compliments, to St. John's College, Fordham, New York, where I was educated.

I have the honor to remain, your obedient servant,

SANTIAGO AINSA.

JOSEPH HENRY, Esq.,
Smithsonian Institution, Washington, D. C.

[This meteorite is now in the museum, and is an object of special interest to visitors.]

LITTLE GLACE BAY, CAPE BRETON, NOVA SCOTIA,
October 25, 1863.

MY DEAR SIR: I send you a specimen of "cone-in-cone," which I have lately obtained in sinking a shaft at this place upon the Harbor Vein seam of coal described in Professor Lesley's report of this coal-field last year.

It was found in the band that corresponds to the black bituminous shales below the one inch of cannel coal, and 23 feet above the Harbor series of five feet of coal.

It was only obtained on the northwest side of the shaft, thinning out to the south and east, or towards the "crop." The greatest thickness of the bed was about 7 inches. The largest "cone-in-cone" was $5\frac{1}{2}$ inches in diameter.

The journal of the strata sunk through differs somewhat from Professor Lesley's taken at the shore.

	ft. in.
At the shaft-drift and gravel	10.0
Blue shales, with cyclas shells, fish teeth, and other remains	3.0
Cone-in-cone	.5
Brown band, with coprolites	.3
Blue arenaceous shales	1.0
Hard white sandstone	2.0
Thin bands of shales "fucoids"	3.2
Hard sandstone	.4
Blue arenaceous shales	2.6
Sandstone, black mark, like the fruit "cardcocarpon"	1.04
Sandy shales	.11
Hard blue shales	3.10
Blacker band	.4
Fire-clay and ironstone balls	7.5
Coal	5.5
	41.11

I cannot find in any work that I possess anything exactly like them, so think they may be of interest to add to your museum.

The points of the cones are downwards.

I shall be glad to hear from you about them after they have been examined. I have sent a specimen to Dr. Dawson, Montreal, but fear the season is too late for him to get it this year.

I remain, my dear sir, your obedient servant,

HENRY POOLE.

Joseph Henry,
Secretary Smithsonian Institution, Washington.

The above relates to a very interesting specimen of a remarkable concretion of a clayey material, which occurs in thin slabs, entirely formed of cones, the axes of which are all at right angles to the parallel surfaces of the slabs. The only explanation which occurs to us of the mode of formation of this structure is that of percolation of water charged with earthy material through a porous rock, and filling a horizontal crevice with parallel sides, with a series of stalactites and stalagmites. J. H.

Hungarian National Museum,
Pesth, October 15, 1863.

Sir: In reply to your esteemed letter of the 29th of May, I have the honor to inform you that the birds sent us through Dr. Flugel have been duly received, and I beg leave to return the heartfelt thanks of our institution for the same. Full acknowledgments have also been made in our reports, and in the newspapers, of our obligations to the Smithsonian Institution, which stands so high in public opinion everywhere.

AUGUST V. KUBINYI, *Director.*

Joseph Henry, Esq.,
Secretary Smithsonian Institution, Washington.

Christiana, Norway, *November* 4, 1863.

Sir: Having been appointed director of the Ethnological Museum at the University of Christiana, I have perused a letter of the 6th May, 1862, from the secretary of the Smithsonian Institution to the secretary of this university.

As this letter alludes to the endeavors of your excellent Institution for the collection of ethnological objects from North America, and the utility of establishing a system of exchange for European curiosities, I have made use of the opportunity to offer you what we have in this line.

The aboriginal population of this country are the Laps or Laplanders, living at present on the mountains and sea-coasts farthest north of Norway, Sweden, and Russia. Their language proves them undoubtedly to be of the Mongolian stock in Asia, and, as such, related to the red man of America. The Laps are a remarkable instance of this race, as they are converted to Christianity and have adopted the habits and industry of civilization, modified by the severity of the arctic climate in their country and their peculiar mode of subsistence as nomads with flocks of reindeer. We have procured a set of models made by the individuals of the people themselves, and illustrative of their present mode of existence.

In offering this small collection for your acceptance, we hope that it may serve a scientific purpose in comparing the red man with his yellow brother in the old continent. If it should be in your power to afford us some corresponding objects from your field of research, that is so immensely more extensive, a

very great desideratum in our collection would be supplied that would engage our most earnest attention.

The articles in question are—

I. Three casts, in plaster, taken from living individuals, viz: 1, an unmixed Lap, 39 years old; 2, a man whose father was a Fin from Russian Finland, and whose mother was a Lap, 42 years old; 3, a man whose grandmother was a Swede, (of the Teutonic stock,) otherwise Lap, 43 years.

II. Four photographic portraits: 1, mixture of Lap and Fin, 28 years; 2, 74 years; 3, 28 years; 4, 38 years—pure Laps.

III. A reindeer, harnessed with its sledge. The sledge is canoe-shaped, so as to be able to move upon the deepest and softest snow without going down into it.

IV. A pair of snow-shoes, being very long pieces of thin wood, with which the Lap can walk upon soft snow. They have straps or stirrups to put the feet into. The man moves on with the staff.

V. A pair of pack-saddles, with which they move their luggage in summer on the back of the reindeers; included is a model of a wooden tub and a cask; two flat pieces of wood to lay across the back of the reindeer are attached.

VI. A trunk, in which is included the wooden bowl for preserving the reindeer milk, and the press for making cheese out of it.

VII. A spade for removing the snow.

VIII. Two large wooden bowls.

IX. A tent; in the middle the fireplace and two pots hanging over it; behind is a scaffolding of wood for their stores, raised upon poles, so that it may not be attacked by dogs.

Confiding in your interest for the advancement of science, I remain, very respectfully, your obedient servant,

LOUIS KR DAA.

JOSEPH HENRY, Esq.,
Secretary Smithsonian Institution, Washington.

[These articles are now in the museum.]

KAISERLICHE-KÖNIGLICHE GEOLOGISCHE REICHS-ANSTALT,
Vienna, December 11, 1863.

SIR: I have the honor to transmit to you for the Smithsonian Institution a series of tertiary fossils from the Vienna basin, viz:

From the Congeria beds	6 species.
From the Cerithium beds	10 species.
From the Marine beds	270 species.
Total	286 species.

In the box prepared to be sent you will find, 1, the present letter; 2, a systematic catalogue, with tabular reference to the localities; 3, a catalogue in which the localities are kept separate; 4, a guide of geographical reference for the localities. The number of specimens or lots in catalogue 3 is 622. Beside these there are a number imperfectly determined or not belonging to Austrian localities. The rest will give a pretty fair idea of the leading or type mollusca of our Vienna basin. The series here offered has been composed or selected under the auspices of Dr. Hörnes, director of the Imperial Museum of Mineralogy, and he placed it at the disposal of our Imperial Geological Institute, so that I beg you will consider it as a joint offer from both establishments.

I have the honor to be, dear sir, ever most truly yours,

W. HAIDINGER.

British Museum, *December* 30, 1863.

Dear Sir: I have to acknowledge the receipt of your letter of this day's date, and to acquaint you that the trustees have acceded to the request made by Professor Henry, on behalf of the Smithsonian Institution, and that I have instructed Dr. Gray to give you every facility with a view to such electrotype impressions being made for that Institution as are required from our wood engravings illustrative of the conchology of the North American continent. I shall be happy to see you, and to give you any assistance in my power whenever it may be convenient for you to call at the museum, as you propose.

Believe me, dear sir, yours truly,

A. PANIZZI.

Dr. P. P. Carpenter.

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31 Pfeidemarket, Hamburg,
February 4, 1864.

Dear Sir: I duly received your very kind letter of the 6th of January, informing me that the director of the Smithsonian Institution would have the kindness to send me five of the American perennibranchiates for investigation. A few days afterwards the box was delivered into my hands, containing—

1. Menopoma Alleghaniense.
2. Menobranchus lateralis.
3. Siren lacertina.
4. Amphiuma tridactylum.
5. Siredon pisciformis.

All these amphibia being of the greatest importance for my studies, I cannot but express to you my most sincere thanks for this most valuable assistance. You will allow me to pay to your renowned Institution, in the mean time, my thanks for the reports and other valuable works, particularly on the Zoölogy and Anatomy of Amphibia, published at Washington, and directed to me some years ago.

I should feel most happy if you would give me a direction how I might pay my thanks in a more material manner. You will, therefore, oblige me very much by informing me of the desiderata in your collections. Perhaps there might be some European fishes or amphibia which I might be able to procure for you. Of sea snakes, which family of snakes I have described some years ago, there are also some few species in my own possession. In minerals I am pretty rich, having the best private collection of this branch that exists in our place.

It is only on the supposition that I might be able to furnish to the Smithsonian Institution some equivalent that I take the great, and, perhaps, immodest liberty to mention, that one specimen more of the genera amphiuma, siren, and menopoma, would be of the greatest importance for my studies. It would be very difficult to decide all the anatomical questions concerning the named amphibia after the investigation of only one specimen. Having the intention to describe in a comparative manner the bones, muscles, and nerves of the famous Salamandra Japonica, with relation to the other genera of Ichthyodea, I feel myself in a high degree advanced by the specimens which I owe to your kindness, and would be induced to hope that my little work might not remain quite imperfect, if there would be any chance to acquire still one specimen more of the above-mentioned three genera.

Finally, you will allow me to say that I am not now in any connexion with the Hamburg Museum, as the address of your letter said, but that, though being on very friendly relations with the directors of our collections, I have given up my place among them.

With the highest regards, I am yours, very respectfully,

Dr. J. G. FISCHER.

[The specimen requested was sent to Dr. Fischer.]

VIENNA, *February* 9, 1864.

MY DEAR SIR: Permit me to enclose here an invitation to join in a subscription for a gold honorary medal to be presented to our most worthy Professor Ch. Fr. Ph. von Martius, of Munich, on his fiftieth anniversary of medical doctorship on the 30th of March, 1864.

Our most honored friends on the other side of the Atlantic should not fail in the list; only I am sorry that by various impediments I was prevented from writing at an earlier period. It is now so late that only by very good luck it will be possible that an answer may arrive previously to the 15th of March, to be entered in the first list which must be printed, embellished, and then bound up, and sent to Munich from Vienna before the 30th of March. Whatever is brought to notice later than the 30th will be appended, and what comes to hand after the 30th up to the end of June will be given in the first complementary report to be published on the 1st of July. Nothing will be lost, as even what comes after that period will be published afterwards.

Every subscriber, of course, will have a bronze copy of the medal, and the votary tablet sent to him. Subscriptions should be three florins Austrian silver money, or more, which is about one and a half dollar American silver.

By this time you may already have received our last box with tertiary fossil types of several localities of the Vienna basin, being a joint parcel from the Imperial Mineralogical Cabinet and our own Geological Institution.

I am happy to hear you have now the Ainsa Tucson meteoric iron. I shall send some of these days a paper of mine on the Carleton Tucson, which appeared in the Vienna Academy Proceedings. I enclose impression from the surface, cut, polished, and etched, and galvanographed positively and negatively. We shall be happy, as soon as you may fix on cutting some slices off the block, to receive a bit from you for our Imperial Mineralogical Museum of the Ainsa Tucson too.

With all the most cordial wishes, ever most truly yours,

W. HAIDINGER.

Professor JOSEPH HENRY,

Secretary to the Smithsonian Institution, Washington.

OFFICE HUDSON'S BAY COMPANY,
Montreal, February 26, 1864.

MY DEAR SIR: Absence from home and subsequent indisposition have prevented my acknowledging receipt of your letter of 19th ultimo at an earlier date.

The settlement you have made of Mr. Kennicott's account is quite satisfactory. There was a small deficiency in consequence of a change in the rate of exchange when your draft reached me; but that matter can be arranged when we receive Mr. Mactavish's final statement of Mr. Kirkby's account.

The kind expressions of thanks contained in your letter are very gratifying. We have always felt pleasure in promoting scientific research; but, in Mr. Kennicott's case, this was enhanced by his amiable character and prudence. It is no easy part to play, going as a stranger into a territory inhabited by men bound to a foreign government, and with exclusive views on many points. But Mr. Kennicott knew how to meet the circumstances; and from his arrival among us until his departure was always popular, and I believe inspired a sincere friendship and esteem among those with whom he most associated. If in Washington, pray offer him my kind regards.

Hoping some day to have the honor and pleasure of forming your personal acquaintance, believe me, sir, very truly yours,

EDW. M. HOPKINS.

JOSEPH HENRY, Esq.,

Smithsonian Institution, Washington, D. C.

GENERAL APPENDIX

TO THE

REPORT FOR 1863.

The object of this appendix is to illustrate the operations of the Institution by reports of lectures and extracts from correspondence, as well as to furnish information of a character suited especially to the meteorological observers and other persons interested in the promotion of knowledge.

LECTURES.

BRIEF ABSTRACT

OF A SERIES OF SIX LECTURES ON

THE PRINCIPLES OF LINGUISTIC SCIENCE,

DELIVERED AT

THE SMITHSONIAN INSTITUTION IN MARCH, 1864.

BY WILLIAM D. WHITNEY, PROFESSOR OF SANSKRIT IN YALE COLLEGE, NEW HAVEN.

THE scientific study of language is of modern date. Only its scanty and imperfect germs are to be found in ancient times. It lacked that wide and comprehensive basis of observed and collected facts on which alone such a science can be founded. The active and searching curiosity of the past century, with the facilities for investigation given by trade, travel, and philanthropic effort, could not but call it into being. No single circumstance has so powerfully aided its development as the introduction of Sanskrit to the knowledge of Europe. This, the most ancient and primitive of Indo-European tongues, laid the sure foundation of the comparative philology of the Indo-European family, out of which has grown the general science of language.

The objects of this science are twofold: To discover the nature and history of language itself, and to elicit information respecting human history. Both are invested with a very high degree of importance. The value of language to man, and the absorbing interest of inquiry into its character, are palpable, and attested by the labors and speculations of generations of scholars and thinkers. It has also quite recently been found that language is the principal means of ethnological investigation, of tracing out the deeds and fates of men during the prehistoric ages. Not only does it determine the fact and the degree of relationship among nations, but it gives information which can be obtained in no other way respecting their moral and intellectual character, and the growth of their civilization. Linguistic science, as a branch of the study of human history, embraces the whole race at every period of its history. All spoken or recorded speech is its material. The dialects of the lowliest as well as the most highly endowed races are its care. It would fain hold up and study every single fact in the light of every other related fact, since only thus can all be fully understood.

To survey in detail, in these lectures, the whole field of linguistic science will be, of course, impracticable. We can only attempt to lay down and illustrate its fundamental principles, to gain some insight into its methods, to determine the nature and force of linguistic evidence, to see how this is elicited from the material containing it, to note its bearing on historical and ethnological study, and to review briefly the principal results hitherto obtained by its means The method followed will be the analytic, establishing principles from facts

within every one's apprehension, and proceeding from that which is well known or obvious, to that which is more obscure. Illustrations will be sought, mainly from among the phenomena of our own familiar speech, since every living and growing language has that within itself which exemplifies the facts and principles of universal application in all language. We shall also avoid, as much as possible, the use of figurative, philosophical, and technical phraseology, and talk the language of plain fact.

Our preliminary inquiry may properly be, Why do we, ourselves, speak English? Though a simple question, its correct answer will clear our way of many difficulties. The general reply is obvious: We learned English from those among whom our earliest years were passed. We did not produce the words we use by an internal impulse, by the reflection of phenomena in our consciousness, and the like. As soon as we were able to associate an idea and its uttered sign, we were taught to stammer the names of the most familiar objects, and our instruction advanced with our capacities; our notions and conceptions were brought into shapes agreeing with those they took in the minds about us, and were called by the names to which these were accustomed. Certain liquids which we saw, colorless and white, had not to be studied and compared by us in order to the invention of a title for them. We were informed that they were "water" and "milk." The one of them, in certain modes of occurrence, we were made to know as "puddle" and "river." The words *cry*, *strike*, *bite*, *eat*, *drink*, *love*, *hate*, and so on, were taught us by being applied to acts and states of which we made experience. Long before any mental analysis of our own would have given us the distinct ideas of *true* and *false*, they were impressed upon our minds by admonition, or something stronger. The appellations of hosts of objects, places, beings, which we had not seen, and perhaps have not yet seen, were fixed in our minds, with the means of attaching some distinctive idea to them. The amount and kind of this training varied greatly in different cases, but we all had it, and by it alone could learn to talk as we do. Language was the first step in our education. It came by education, and not by inheritance. English blood would never have given us English speech. We could just as easily have learned to say *wasser* or *eau* as "water," *milch* or *lait* as "milk," *lieben* or *aimer* as "love," &c. An American child is brought up by a French nurse in order that it may speak French first, and it does so. The infant cast on shore alive from a wreck learns the tongue of its foster-parents, and no outbreak of natural speech ever betrays whence it derived its birth. The imported African forgets, in a generation, his Congo or Mendi, and is able to use only a dialect of his master's speech.

It is already clear, then, that English people do not, as some have paradoxically maintained, speak English by inherent natural gift, because they are English, just as all swallows twitter, all bears growl, all lions roar, and so on. The special forms of spoken language are matters of imitation. They are kept up by usage, and transmitted by oral tradition.

We thus learn, not English simply, but the particular kind of English which is spoken by our instructors. A few, perhaps, get nothing from the outset but the purest style of the language; but hardly any can escape some tinge of local dialect, of the slang of caste or calling, even of individual peculiarities of our teachers, inelegancies of pronunciation, pet phrases, colloquialisms and vulgarisms, and the like. Often errors and infelicities thus acquired in early life are ineradicable by all the care of after years.

Again, this process does not give us universal command of the resources of the language. A child's vocabulary is very scanty, and goes on increasing to the end of life. The encyclopedic English tongue, as we may call it, contains over one hundred thousand words. Of these, the most uninstructed classes acquire only three to five thousand, a frugal stock of the most indispensable words and phrases. To such a nucleus every artisan, in every walk of labor, must

add his own technical language, containing much which most English speakers know nothing of. No small portion of the one hundred thousand words is made up of such special vocabularies. The generally educated man learns much of many of them, but no one learns them all. Every one may find, on every page of our great dictionaries, words which he knows not how to deal with. There are various styles of expression for the same thing which are not at every one's command. Even the meanings attributed to the same words by different speakers are different. The voluptuary, the passionate, the philosophic, and the sentimental, for example, mean very different things by "love" and "hate." It is no paradox to maintain that, while we all speak English, no two among us speak precisely the same language, the same in extent, form, or meaning.

What, then, is the English language? It is the aggregate of the articulated signs for thought current among the English people; or, it is their average, that part which is supported by the usage of the majority—a majority counting not by numbers only, but by culture. It includes varieties of every kind; but it has unity, from the fact that all who speak it may, to a considerable extent, and on matters of the most general interest, talk so as to understand one another. It is kept in existence by uninterrupted tradition, in which each individual takes a part, handing down his portion of it, with his limitations and peculiarities—books, a kind of undying individual, greatly assisting in the process. But all traditional transmission is inherently and necessarily defective, and that of language forms no exception. If English were a certain fixed body of words, learned complete by every one, and kept intact, it might more easily be preserved from alteration. As the case stands, it does not remain the same from generation to generation.

Its most noticeable mode of alteration is that which is ever going on in its vocabulary, especially its technical vocabularies. New processes and products, new views and opinions, new knowledge of every kind, must find their fit expression. No well-informed man can write a chapter now upon what every one is thinking and talking of which would be intelligible to the well-informed man of a century ago. There are also changes affecting rather the form than the content of language, of slow progress, and in their inception, in great part, inaccuracies of speech, opposed by the conservative forces, yet as inevitable in the end as the others. They show the influence of the great numerical majority who do not speak with correctness, but whose errors finally become the norm of the language. Thus, we had formerly a special preterit form *spake*, and good speakers would as soon have said "he come and done it" as "he spoke to me." Now only *spoke* is in common use. Three centuries ago we had only *his* as possessive of both *he* and *it*, but popular usage struck out a new possessive, *its*, for the latter. *You* we employ not only as object, according to its ancient usage, but as subject, instead of *ye*, &c., &c. The influences which brought about such changes are still to be seen in full operation about us, especially among children and uninstructed persons, to whom the communication of the language is imperfectly or incorrectly made. A child substitutes an easy for a hard sound in pronouncing, drops out a syllable or two from a half-understood word, says "I bringed" or "I brang" for *I brought*, says "mans" and "mouses," says "gooder" and "goodest," and the like. Its own and others' care corrects these errors; but if the care be wanting, the error remains; and there are ever in existence, among the lower strata of language-users, hosts of these deviations from correct usage, always threatening, and sometimes succeeding in making their way to the surface, and securing recognition and general adoption. The conservative forces arrayed against them, aided by school instruction and reading, are now so powerful among us that the language changes but very slowly in this way, yet the examples given are truly typical, and illustrate a force always in action. That, in these and other methods, language actually undergoes notable change is palpably true. Go back only to

our Bible translation, to Shakspeare, and much is found which is no longer good English. Go back five hundred years, to Chaucer, and our own tongue is only partially intelligible to us. Another five hundred years carries us to the Anglo-Saxon of King Alfred, a totally strange form of speech, as much so as the modern German; and yet each one of the thirty or forty generations between us and Alfred was as singly intent on transmitting to its successor the language it received from its predecessor as is our own.

These facts and conditions are of universal occurrence in linguistic history. All language is handed down in the manner described, and is subject to the same disturbing forces. The process of transmission always has been, and always will be, imperfect. No tongue remains the same during a long period of time. This is the fundamental fact on which rests the whole method of linguistic investigation.

We see now what is meant when language is spoken of as having an independent existence, as being organic, or an organism, as growing or developing, and so on. These are only figurative modes of speech. Language has no existence, save in the minds and mouths of those who employ it. It is an aggregate of signs of thought, deriving their significance from the intelligent agreement of speakers and hearers. It is in their power, and subject to their will. As they maintain it in existence, so their consenting action modifies and alters it. It cannot be changed hastily or capriciously, because it depends upon general consent, which can be won only for such modifications and extensions as are in accordance with its already established rules. Individuals are constantly trying experiments of alteration upon it, with childish errors of expression, with bad grammar, with slang, with artificial turns of phrase, and arbitrarily coined words. But these are, for the most part, only laughed at as blunders, or put down as mannerisms and vulgarisms. Individual authority, except in special cases, is too weak to force itself upon public opinion. The speakers of language constitute a republic, in which authority is conferred only by universal suffrage, and for due cause. High political rank does not give power over speech. The grammatical blunders of an emperor do not become the rule to his subjects. But individuals are allowed to introduce novelties and changes into the general speech; thus, for instance, to name their own inventions or discoveries, if they do it discreetly and suitably; and great masters of the art of speech, poets, orators, are permitted to touch even the more intimate and sacred parts of language. Is it called for? is it in accordance with the usages and analogies of the language? is it offered or supported by good authority?—such are the considerations by which, in any given case, general consent is won or repelled, and this decides whether the proposed change shall be rejected, or shall become part and parcel of the universal speech.

As, then, an organic being grows by the gradual accretion of homogeneous organic matter, as its existing parts and processes form the new addition, in order to help the life and functional action of the being, so language extends by the addition of material accordant with its substance, evolved by its formative methods, and intended to secure the end of its existence, the expression of the thoughts of those who speak and write it. It thus presents striking and instructive analogies with organic life; but to call it an organism outright, as some do, and to claim that its growth is independent of human agency, and that its study is, therefore, to be ranked among the physical sciences, is palpably and seriously to misinterpret it. Language is an institution, constantly undergoing, at the hands of those who use it, adaptation to their varying circumstances and needs. Between all determining causes and their results in its development stands, as middle term, the human mind, seeking and choosing expression for human thought. Its every part is a historical product. Its study is a historical science, a branch of the study of the human race, and of human institutions.

As every constituent item of language is the product of a series of changes, working themselves out in history, the method of linguistic investigation must be historical. To understand the structure and character of speech, and to penetrate to its origin, we must follow backward the modifying processes to which it has been subjected, endeavoring to understand the influences which have produced and governed them. This can be done to but small extent by means of contemporary records. We must call to our aid the art of etymological analysis. On etymology, the tracing out of the history of individual words, is founded the whole science of language. To illustrate the methods of etymologizing, and to bring to light some of its results, by simple and characteristic examples, is the object of this second lecture.

Let us look first at evidence showing the composite nature of words. We are all the time putting together two words to form a compound; as, *fear-inspiring*, *god-like*, *house-top*, and so on. But the extent to which language is the result of such composition is apparent only on deeper study. *Fearful* is as clear a compound, on reflection, as *fear-inspiring;* yet *ful* is, to our apprehension, a kind of suffix, forming a large class of adjectives from nouns, like the suffix *ous*, (in *peril-ous*, *riot-ous*, &c.;) and its independent origin and meaning are but dimly present to the mind of one who uses the adjectives. *Fearless* and its like are not less evident compounds; but the *less* here is not our word *less*, but the altered form of an older word, meaning "loose, free." Again: *ly*, in *godly*, *brotherly*, &c., is of yet obscurer origin, and we deem it merely a suffix; but a study of the other forms of our language, or a comparison of kindred Germanic dialects now spoken, shows it to be descended from the adjective *like*, which has been used in all the languages of our family as an adjective-forming suffix; we alone have given it the further and now remotely derived office of adverbial suffix, employable at will to convert any adjective into an adverb. The *d* of such words as *I loved*, *I hated*, is proved by the form it wears in the oldest Germanic tongues to be a relic of the past tense *did*: *I loved* is originally *I love did*. *Such* and *which* were once *so-like* and *who-like*, and so on. The same is the case in the Latin part of our language, and even in its oldest and most essential constituents. The *ble* or *ple* of *double*, *triple*, and so on, is the root *plic*, meaning "bend, fold;" *triple* is the precise etymological equivalent of *three-fold*. The two letters of *am*, which seems as simple a word as aught can be, are relics of two elements: one, the root *as*, meaning "be;" the other, the pronoun *mi*, meaning "me, I;" *am* stands for *as-mi*, "be-I." The third person, *is*, has lost the whole of a second element, *ti*, which it once possessed, and of which at least the *t* is left in nearly all the kindred languages; compare German *ist*, Latin *est*, Greek *esti*, Sanscrit *asti*, &c.

With few exceptions, all the words of our language admit of such analysis, which discovers in them at least two elements: one radical, containing the fundamental idea; the other formal, indicating its restriction, application, or relation. This is, in fact, the normal constitution of a word; it contains a root and a suffix or prefix, or both, or more than one of both. Thus, *inapplicabilities* contains two prefixes and three suffixes, all clustered about the root *plic*, "bend;" and it is, as it were, the fusion and integration of the phrase "nume rous conditions of not being able to bend or fit to something."

Our examples show that word-analysis is, at least in part, only the retracing of a previous synthesis. We are as sure of the actuality of the process of combination by which these words were formed as if it had all gone on under our own eyes. There would have been no such suffixes as *ful*, *less*, *ly*, &c., if there had not been before in the language the independent words *full*, *loose*, *like*, &c. No small part of the formative elements of our language can thus be proved descended from independent words; if a considerable part do not admit like proof, we are not authorized to suppose that their history is different

from that of the others, but only that we have not at command the evidence which would explain it.

The same examples show not less clearly that alteration, corruption, and mutilation of the products of combination is a rule of the life of language. The reason of this corruption lies in great measure in the fact that, having once struck out a compound, we are not solicitous to keep up the memory of its descent. We accept the word coined as a conventional sign for the idea which it conveys, and give our attention mainly to that. Hence ease and convenience in the use of the word are consulted; a long vocable is contracted; a hard combination of consonants is mouthed over into more utterable shape; subordinate elements are defaced into conformity with the inferiority of their consequence. So the sailor says *bos'n* for *boatswain*, *to'gal'nts'ls* for *topgallantsails*, &c. This is a part of the wise economy of speech, a sign and means of the integration of words, contributing to conciseness and vigor of expression. But it is also a blind tendency, and its effect is in part destructive. It leads to waste as well as economy; ease and convenience being consulted by the sacrifice of what is valuable as well as the rejection of what is unnecessary—if, indeed, it can truly be said that a people not undergoing degradation of character ever sacrifices anything of its language which is really valuable without providing an equivalent. A language may thus, at any rate, become greatly altered, giving up much which in other tongues is retained and valued. Our own English offers one of the extremest examples known of the prevalence of these wearing-out tendencies.

Thus, for instance, the primitive language from which our own is descended had a full set of terminations for the three persons plural of the verb, viz: *masi*, *tasi*, *nti*—e. g., *lagamasi*, *lagatasi*, *laganti*, "we lie, ye lie, they lie." In Latin they appear shorn of their final vowel, as *mus*, *tis*, *nt*. In Gothic, the oldest Germanic language, they are reduced to their initial consonants only, *m*, *th*, *nd*—thus, *ligam*, *ligith*, *ligand*. They are still, in this form, pretty distinctive, and sufficient for their purpose. But the prevailing custom of expressing the pronouns along with the verb lessened their necessity; and in Anglo-Saxon they are all reduced to a single form, *ath* in the present, *on* in the imperfect. We, finally, have cut them off entirely, and say *we lie*, *ye lie*, *they lie*, without any endings designating the person.

In the declension of nouns we have effected a revolution not less thorough. Our ancient mother-tongue declined every noun substantive in three numbers, with eight cases in each, and every adjective in three genders besides. With us all adjective declension has disappeared, and of substantive declension we have saved only a genitive and a plural ending, both *s*. In a few plurals, as *men*, *mice*, *teeth*, we have seized upon a distinction at first euphonic and accidental only, and have made it significant. So also in the conjugation of our "irregular" verbs, as *sing*, *sang*, *sung*; the change of vowel was at first merely euphonic, then became, as in most German dialects it still continues, auxiliary to the sense, and finally, with us, it is in many cases the only means of distinction of present, preterite, and participle.

In one remarkable case, the wearing-out processes have led to the total abandonment of a conspicuous department of grammatical structure. A distinction of gender in nouns, as masculine, feminine, or neuter, marked by differences of termination and declension, has ever prevailed in the family of languages to which ours belongs. Even in the Anglo-Saxon, nouns were still masculine, feminine, or neuter, not according to their natural character, but in conformity with the ancient tradition, on fanciful grounds of difference, which we find it excessively difficult to trace out and recognize. But in the extensive decay and ruin of grammatical forms attending the elaboration of modern English from Anglo-Saxon and Norman French, this whole scheme of artificial

distinctions has disappeared, leaving almost no trace behind. Natural gender has replaced grammatical, and the pronominal forms *he, she, it, his, him, her, its,* are our only means for its indication.

These two processes—the production of new forms by the combination of old materials, and the wearing down and wearing out of the forms so produced, are the principal means by which the external life and growth of language are kept up, by whose operation spoken tongues are constantly becoming other than they were. But they are only auxiliary to a not less striking growth in the interior content of speech, in the meaning of words. It is as important a part of the historical study of a word to trace out its changes of signification as its changes of form; and the former are even richer in curious and unexpected developments, are fuller of instruction, than the latter. The internal content of language is plastic to the touch of the inspiring mind. But for this, no variability of form or facility of combination could make it aught but a stiff dead structure, incapable of supplying for any time the needs of a thinking, feeling, observing, and reasoning community. Old words are applied to new uses; the general is individualized, the individual generalized; the concrete becomes the abstract; a pregnant expression, a startling metaphor, is reduced to the level of an ordinary phrase; delicate shades of meaning are distinguished by the gradual differentiation of synonymous words, and so on.

The rate at which these processes of change go on is very various. It depends, in part, upon subtle and recondite causes, as upon the individual character of different languages and the qualities of the peoples who speak them—qualities, perhaps, which exhibit themselves only in this way, and hardly admit of analysis and recognition elsewhere. In part, it depends also upon external circumstances, upon change of surroundings and mode of life, of mental and physical activity. An English family, wrecked on a coral island in the south seas, would soon find a great part of its vocabulary useless, and in a very few generations its language would have become vastly impoverished. A tribe from such an island, again, if suddenly transferred to the midst of northern variety of clime, product, and occupation, would have to expand rapidly its store of speech to keep pace with the growing wealth of its experiences. As regards grammatical change, all that assists the purity of linguistic tradition tends to keep language the same; so, especially, culture, literature, the habit of instruction. Careful and pervading education reduces to a minimum that immense and most important class of changes which begins in popular inaccuracies. On the other hand, the intermixture of races of diverse speech, rendering necessary the elaboration, by mutual compromise, of a new dialect for common use, tends powerfully to the disorganization of grammatical structure. It is such a course which has made of our English the language which, above all others, has yielded up most of the grammatical fabric which was its birthright and inheritance.

The processes of alteration illustrated in the last lecture are familiarly spoken of as going on in language itself, like fermentation in bread, or deplacement and replacement in animal tissues. But it must not be forgotten that every separate item of change is the work of an individual or individuals. In language, the ultimate atoms at work are not dead matter, but intelligent beings, acting for a purpose. Each, indeed, acts unpremeditatedly, and for the most part unconsciously; each only wants to use the common possession for his own benefit, at his own convenience; yet each is also an actor in the great work of preserving and of shaping the general speech. Now, the infinite diversity of circumstances and of characters in the speakers of language tends toward infinite diversity in their action and its results; each would, acting independently, impress upon its progress a somewhat different course. Linguistic develop-

ment is thus the product of an infinity of divergent or centrifugal forces. The great centripetal force which holds them in check, and combines them into a single direction, is the necessity of communication. Man is no soliloquist, and that would not be language which was understood and employed by one only. Each person is, in his own way, engaged in modifying language, but no one's action shapes the general speech unless it be accepted by the rest and become common usage. Each community must speak alike; whatever changes their tongue may undergo must be ratified and adopted by them all.

Communication being thus the force which produces uniformity of speech, it is clear that whatever narrows communication and tends to isolate communities favors separation of a language into dialects; whatever extends communication and expands the limits of communities, tends to preserve language homogeneous. When a race is confined within narrow boundaries, however rapidly its tongue may undergo the inevitable processes of change, all will learn from each and each from all, and they will continue to understand one another. But if the race grow rapidly in numbers, spreading over region after region, and sending out distant colonies, only favoring circumstances and conditions can preserve its unity of speech. In a low state of civilization a maintenance of the bonds of community over a wide area is impracticable; the tendency is to clannish feeling, to separation into tribes; and multiplicity of dialects is the natural consequence. Culture and enlightenment give a wonderful cohesive force; political unity, national feeling, community of traditions and faith, make strongly in favor of linguistic unity also; a traditional literature helps yet more powerfully to the same result; but, most of all, a written literature, and a system of popular instruction. The same causes which restrict the variation of language in time, from generation to generation, restrict it also in space, from region to region. Moreover, as community occasions and preserves identity of speech, so it also has power to bring identity out of dissimilarity. The fusion of communities causes the fusion of their forms of speech; the multiplication and strengthening of the ties which bind together the sections of a people makes for the effacement of differences already existing, the assimilation of dialects, and the production of homogeneous language.

Both classes of influences—those which lead to diversity and those which produce assimilation—are always at work, and a consideration of their joint and mutual action is necessary to the explanation of the history of any language, or family of languages; but the former are more fundamental and inseparable from linguistic growth; the latter are more external and incidental, more varying in their mode and scale of operation. Language everywhere tends to diversity, but circumstances connected with its use check, control, and even reverse the tendency. The division of a formerly homogeneous language into dialects has been the rule in human history; the extinction of dialectic differences, whether by the extinction or fusion with others of the peoples employing them, or by extension of the sway of single dialects, has been the exception, connected with the great facts of history, as the spread of empire and civilization under the auspices of certain races. Misled by a too exclusive attention to facts of the latter class, one or two modern authors of high rank have been guilty of the paradox of holding that infinite dialectic division is the normal primitive state of language, which tends to coalescence and assimilation. A greater and more pernicious error could hardly be maintained.

The principles here laid down teach us how we are to proceed in classifying and arranging the infinity of tongues now prevailing on the earth. Many of them, at least, are the divergent branches of more original stocks. Languages are to be grouped by their affinities: we are to rank together first those which

are of closest and most evident relationship, and gradually to extend our scheme till we have done all which the nature of the case permits; till the evidence on which we found our classification fails us.

That the slightly distinguished forms of speech prevailing in the different sections of our own country, and even the more notable dialects which are to be found among the lower orders of population in the British isles, constitute together a single language, is too evident to call for proof. Let the man most ignorant of history go about the world, from British colony to colony, finding here and there, on coast and island, in fortress and city, communities of English-speaking people, and he will not think of doubting that they were all scattered thither from a common centre, and have their common language by community of linguistic tradition. A like conclusion is almost equally palpable when we seek after kindred for our language on the continent of Europe. There is a large class of evidently related dialects, occupying the Netherlands, Germany, Denmark, the Scandinavian peninsula. and Iceland, which a very little study shows us to be akin with the more important half of our own tongue, that which comes to us from the Anglo-Saxon. There is another large class in southern Europe, comprising the French, Spanish, Portuguese, Italian, Rhæto-Romanic, and Wallachian, which exhibit an equally clear connexion with the non-Saxon part of our familiar speech. If we say *true*, while the Dutchman says *trouw*, the German *treu*, the Swede and Dane *tro*, &c., it is because we have all received the same word in the same sense by uninterrupted tradition from some community which used a form coincident with one of these, or nearly resembling them all. So, also, if we say *verity*, while the Frenchman says *vérité*, the Italian *verità*, the Spaniard *verdad*, &c. Recorded history, in fact, fully explains the descent of this latter class of languages from a single mother, the Latin, as it also makes clear why our English is composed of materials derived from both classes. What recorded history does not explain is the more recondite, but not less undeniable evidence of relationship which we discover between these two classes themselves, as well as between them both and most of the other languages of Europe, together with some of those of Asia. These are, namely, the Greek, ancient and modern; the Slavonic, occupying Russia, Poland, Bohemia, Servia, and other provinces in the eastern part of Austria and the northern of Turkey; the Lithuanic, around the southern shore of the Baltic; the Celtic, of which the scanty remains are now found in Ireland, the Scotch highlands, Wales, and Brittany; and, outside of Europe, the tongues of Iran, as the Persian, with its ancient and modern congeners, and its remoter kindred, Kurdish, Armenian, Afghan, and Ossetic; and, finally, the languages of India, the Sanscrit and its descendants.

These various branches go together to make up the great family of related languages which we call the Indo-European. Their relation to one another is the same in kind with that of the various Germanic dialects, or the Romanic, and differs only in degree. The resemblances and coincidences which they exhibit are explainable only upon the hypothesis of a common linguistic tradition; their differences are fully accounted for by their divergent growth and development during the ages which have passed since their separation. A few selected specimens of their accordance will be enough to give here, as their relation is now a matter of general knowledge, and few or none are found to doubt or deny it. Examples of words corresponding in all or nearly all the branches are as follows (the equivalent words in two or three unconnected languages are also added for the sake of more fully exhibiting the value of the coincidences):

	Two.	Three.	Seven.	Thou.	Me.	Mother.	Brother.	Daughter.
Germanic	Twa.	Thri.	Sibun.	Thu.	Mik.	Muoter.	Brothar.	Dauhtar.
Lithuanic	Du.	Tri.	Septyni.	Tu.	Manèn.	Moter.	Brolis.	Dukter.
Slavonic	Dwa.	Tri.	Sedmi.	Tü.	Man.	Mater.	Brat.	Dochy.
Celtic	Dau.	Tri.	Secht.	Tu.	Me.	Mathair.	Brathair.	Dear.
Latin	Duo.	Tres.	Septem.	Tu.	Me.	Mater.	Frater.	....
Greek	Duo.	Treis.	Hepta.	Su.	Me.	Mētēr.	Phrator.	Thugatēr.
Persian	Dwa.	Tri.	Hapta.	Tūm.	Mē.	Mātar.	....	....
Sanscrit	Dwa.	Tri.	Sapta.	Twam.	Mē.	Matar.	Bhrātar.	Duhitar.
Arabic	Ithn.	Thalāth.	Sab'.	Anta.	Anā.	Umm.	Akh.	Bint.
Turkish	Iki.	Üch.	Yedi.	Sen.	Ben.	Ana.	Kardash.	Kiz.
Hungarian	Ket.	Harom.	Het.	Te.	Engem.	Anya.	Fiver.	Leany.

But, to the historical student of language, correspondences of grammatical structure are more unequivocal signs of near relationship than correspondences of words, being less exposed to imputation of accidental origin. As striking and convincing an example of this kind of evidence, perhaps, as any other is furnished in the inflection of the verbal tenses, as follows:

	I have.	Thou hast.	He hath.	We have.	Ye have.	They have.
Germanic	Haba.	Habai-s.	Habai-th.	Haba-m.	Habai-th.	Haba-nd.
Lithuanic	-mi.	-si.	-ti.	-me.	-te.	-ti.
Slavonic	-mi.	-si.	-ti.	-mu.	-te.	-nti.
Celtic	-m.	....	-d.	-m.	-d.	-t.
Latin	Habeo.	Habe-s.	Habe-t.	Habe-mus.	Habe-tis.	Habe-nt.
Greek, (dialectic)	-mi.	-si.	-ti.	-mes.	-te.	-nti.
Persian, (modern)	-m.	....	-d.	-m.	-d.	-nd.
Sanscrit	-mi.	-si.	-ti.	-masi.	-tha.	-nti.

These are specimens, taken from among a host of others which crowd every part of the grammar and vocabulary of the languages in question, and their convincing weight it is impossible to deny. It is certain that at some time in the past, and in some limited region of Asia or Europe, there lived a tribe from whose rude speech have descended all those rich and cultivated tongues now spoken and written by so many great nations of both the eastern and western continents; but to know just where and when is beyond our power. The claim often set up that the home of the family was in the northeastern part of the Iranian plateau, not far from the mountains of the Hindu-Koh, rests upon no sufficient grounds. The traditions of no race reach back far enough to be authoritative upon such a point. Nor is the testimony derivable from language more conclusive. And to define, even with distant approach to confidence, the time which the tongues of the family must have occupied in running their career of development is wholly impracticable. That the time of Indo-European unity must have been thousands of years before Christ is very certain. Recent discoveries are proving that man's antiquity is much greater than has hitherto been usually supposed. Respecting the origin of particular races our knowledge is likely ever to continue exceedingly indefinite. As to the grade of civilization and mode of life, however, of the Indo-European family before its dispersion, their language gives us reliable, though incomplete, information. Words which are found in the speech of all the separated branches must have appertained to the mother tongue, and must imply the knowledge or possession, in that primitive period, of what they indicate. By such means we learn that the tribe was not nomadic, and that it addicted itself to agriculture and the raising of cattle. It reared our chief domestic animals. The region it inhabited was varied, and not near the ocean; its most marked season was winter. Barley, and perhaps wheat also, was raised for food. Certain metals were worked, perhaps iron among them. Weaving was practiced. The arms of offence and defence were those usual among primitive peoples—the bow, sword,

spear, and shield. Boats were built and managed by oars. The political organization was probably that of petty tribes. The relations of the family were well and distinctly established. Some of the stars were noticed and named; the moon was the chief measurer of time. The religion was polytheistic—a worship of the personified powers of nature, and its rites were practiced without a priesthood.

The present lecture is to be devoted to the further consideration of the Indo-European family, to a brief exposition of its importance, and of the special interest attaching to its language, and to some account of the history of the latter.

One source of the especial interest which we feel in Indo-European speech is found in the fact that our own language is one of its branches. This would call for and justify a particular attention to it on our part, even did it lack claims to the same from men of other races. But it does, in fact, possess such claims, and that partly by reason of the historical importance of the peoples which speak it, and their superior gifts, which lend prominent value to inquiries into a matter which illustrates both. Since the first rise of the Persian empire, the various branches of this family have borne a leading part in the drama of universal history. Greece, however, the bitter foe and final conqueror of Persia, was the chief founder of Indo-European greatness, and the most brilliant example of Indo-European genius; in art and literature what the Hebrew race has been in religion, and exerting an influence as unlimited in space and in time. Rome next, inheriting the fruits of Greek culture, gained the empire of the world, and impressed upon all nations a political and social unity. Christianity itself, rejected by the Semitic race among whom it appeared, was taken up by Indo-Europeans, and added a new bond of unity, a religious one, to the ties by which Rome bound the world together. The Germans were mainly instrumental in overthrowing the power of Rome; they gave monarchs to nearly every throne in Europe, and infused new blood into the effete populations; but their devastations ushered in a period of darkness, during which it seemed for a time as if the Semites, inspired with the fury of a new religion, (Mohammedanism,) were to succeed to the empire of humanity. With their repulse and downfall began the last and most glorious era of Indo-European supremacy, in the midst of which we live; when the races of that family are the undisputed leaders, the acknowledged guardians and propagators of civilization. The establishment of the unity of this family, and the light thrown from language upon its history, constitute the most brilliant achievement of the new science of language, which began with its recognition, and has developed along with its investigation. Indo-European language furnished such a grand body of related facts as the science needed for its sure foundation. Its dialects have a range, in period and variety of development, to which those of no other family approach; they illustrate the processes of linguistic growth upon an unrivalled scale. The records of Chinese literature go back, perhaps, to an antiquity as great, or greater; but the Chinese language is almost without a history. There are Egyptian written documents which are older than anything else the world has to show, but they are scanty and obscure, and the Egyptian tongue also stands comparatively isolated. The Semitic languages come nearest to offering a parallel; but they, too, fall far short of it. While their age is nearly the same, their variety is greatly inferior; they are a group of closely related dialects, not presenting greater differences than some single branches of the Indo-European family, as, for instance, the Germanic. And the other divisions of the human race hardly cover, to any notable extent, time as well as space with their known dialects; they offer us only their extant forms of speech. Now, much may be

done, even with the aid of contemporary related dialects only, toward penetrating their common history, because one will be found to have preserved one part, another another, of their ancestral tongues; but conclusions so reached will be inferior both in copiousness and in certainty to those which are derived from a comparative study of older and younger dialects, which illustrate the laws of change in their progress, and trace, as it were, currents and courses of development whose direction we can follow backward with confidence. This advantage we enjoy, to the highest known degree, in the Indo-European languages. In the Germanic branch we have several different lines of linguistic descent, extending through a period of 1,500 years; the English going back to the Anglo-Saxon of the seventh century; the German nearly or quite as far; the Scandinavian to a somewhat less remote period; while the venerable Gothic of the fourth century (oldest of all) helps notably to bridge over the interval to the primitive language of the family. Celtic literature is much less rich, and also less ancient, carrying us up to or beyond the tenth century. The oldest of the numerous Slavonic dialects, the ancient Bulgarian, has monuments a thousand years old. The Lithuanic is of much more recent date, but in many of its forms more antique and primitive than any of the languages hitherto referred to. The Romanic languages, through their mother, the Latin, take us up to a few centuries beyond the Christian era; the Greek to toward a thousand years before Christ. The varied series of Persian tongues comes down from an antiquity nearly equalling the Greek; and the Sanscrit, the sacred language of ancient India, exceeding all the rest in age, and yet more in its preservation of primitive material and forms, reaches in its oldest records an epoch removed nearly 4,000 years from our own day.

In investigating this rich and varied body of kindred tongues, the new science of language elaborated its processes and deduced its general laws, applicable, with such modifications as the separate cases require, to other families also. The general method of study is everywhere the same, being conditioned by the nature of language itself, as a thing of historic growth, and by the capacity of related languages to cast light upon each other's history. Historic analysis, by the aid of an extensive and careful comparison of kindred forms, is the grand means of research. From this its fundamental method, the science, in its growing stage, bore for some time the familiar name of "comparative philology." The comparison must be made in a scientific and orderly manner, proceeding from the nearer to the more remotely connected, from the clearer to the more obscure; but, finally, all language is brought within its sphere, and the full meaning of each linguistic fact is read in the light of every other, diverse as well as correspondent.

The history of Indo-European speech has been more carefully read, and is better understood, than that of any other grand division of human language—imperfect as is still our comprehension of much that concerns it, partly owing to the incomplete analysis of evidence still preserved, but partly also to the irreparable loss of evidence. Some of the principal facts in that history are worthy of further attention.

The chief processes in the growth of the languages of our family have been shown to be the combination of old material into new words, with accompanying corruption and mutilation of phonetic form and independent meaning. These processes may go on in the future to an indefinite extent, with constant evolution from each form of speech of another slightly differing from it, until the descendants of every existing dialect shall be so unlike their ancestors that their relationship shall be scarcely discoverable. The question arises, whether there has been the same indefinite progress in the past, without traceable sign of an actual beginning. This inquiry is to be answered in the negative; the evidence of language points distinctly back to an earliest condition, or commencement of history; our analysis brings us finally to elements

which we must regard as original. First, it must be claimed that our analyses are real, and not imaginary; they are the retracing of the steps of a previous synthesis. This is palpably the case with the latest of them, as in the case of *truthful (truth-ful)* and *godly (god-like)*; it is equally clear, too, as regards all the formative apparatus which is peculiar to the Germanic languages, since this must have been elaborated by them from their own materials, since the separation of the Germanic branch from the rest of the family. But there is no stopping in this series of admissions. Every word-element, separable by analysis, of which the genesis can be shown, which can be carried back to a word having an independent status in the language, must have been appended as an independent vocable to the words with which it was first connected. And even more. Considering how easily the evidence of origin becomes obliterated by the processes of phonetic alteration, we may not deny a former independence to formative elements of which we cannot now trace the genesis. The parts into which etymological analysis separates our words are, as a universal rule, those by the actual putting together of which the words in question were once made up. In analyzing *irrevocability*, for example, we take off affix after affix, leaving each time a word to which that affix had been added, till at last is left only the syllable *voc*, which conveys the idea of "calling," and which, though nowhere appearing in its naked form in actual use, we must believe to have existed before any one of the various affixes with which we find it in combination was appended to it. To such syllables, which we call roots, we everywhere arrive by pushing our analytical process to the utmost, and these we believe to be the germs out of which language has actually grown. In other words, the Indo-European languages began with an original monosyllabic stage. From monosyllabic roots, by processes not differing in nature from those which are still in operation, has been developed the marvellous and richly varied structure of our modern speech. This is a truth, the recognition of which has been reached, almost with unanimity, by students of language; the objections which are urged against it by the few who refuse it their belief are founded in misapprehension and prejudice, and are of no avail.

The Indo-European roots are of two classes: roots of position, demonstrative or pronominal roots, and roots of quality, predicative or verbal roots. The former form chiefly pronouns and prepositions; the latter, verbs and nouns. Pronominal roots denote the relations of things to the speaker as regards place; their fundamental distinction is between the *this* and the *that*, the nearer and the remoter object. They are of the simplest phonetic form, generally a simple consonant with a following vowel, composing an open syllable, and they are but few in number. The verbal roots are more numerous, counting by hundreds, and they are of every variety of form, from a simple vowel to a vowel both preceded and followed by one or more consonants. Instances are: *i* and *gá*, denoting simple motion; *ak*, swift motion; *stá*, standing; *vas*, staying; *sad*, sitting; *pad*, walking; *vart*, turning; *pat*, flying; *ad*, eating; *pá*, drinking; *vid*, seeing; *vak*, speaking; *dá*, giving; *garbh*, grasping; *dik*, pointing out; *bhar*, bearing; *kar*, making; *bandh*, binding; *bhá*, shining; *bhú*, growing, &c., &c. They represent each its own meaning in its nakedness of all limitations or applications, in a state of indeterminateness from which it is equally ready to take on the semblance of verb, substantive, or adjective.

The first beginnings of polysyllabism were made by compounding together roots of the two classes. Thus, the addition to the root *vák*, "speaking," of the pronominal elements *mi*, *si*, *ti*, produced combinations to which usage assigned the meaning "I speak, thou speakest, he speaks," laying in them the same idea of predication which we put into the ambiguous word *love*, when we say "I love." Other pronominal elements, modified or combined to express

duality and plurality, formed the other numbers of this simple verbal tense. The prefixion of an augment, an adverbial prefix, pointing to a "then" or "there" as one of the conditions of the action, gave a past tense; reduplication, symbolizing the completion of the action, produced a perfect. The future and the moods, subjunctive and optative, were chiefly formed by composition with the developed forms of other roots, signifying "to be" and "to desire." Expansions of the verbal scheme, down to such late formations as the Germanic preterit *(I love-d = I love-did)* and the Romanic future (*j'aimer-ai = j'ai à aimer*, "I have to love,") are very numerous and various. The same root of action or quality, by the addition of other affixes, in part of pronominal origin, in part derived from other verbal roots, had its indefiniteness limited to expression of the person or thing possessing the quality or exerting or suffering the action, or of the act or quality itself; and the forms so created became the basis of still further modification and combination. Thus arose nouns, substantive and adjective; for the two classes are originally and in idea but one. Things were named as the possessors of qualities or acts, not in the way of definition or complete description, but by seizing on some notable characteristic, and making it stand as representative of the rest. Nouns were provided with case-terminations; these varied the themes to which they were appended, as to number, whether singular, dual, or plural; as to gender, whether male, female, or neither of the two, (and this, as already noticed, upon an ideal scheme of classification;) and as to case, or kind of relation sustained to the action of the sentence, whether as subject, direct object, or indirect object, with implication of the relations which we express by the use of the prepositions *to, in, with, from, for*, and *of*. Eight such cases were possessed by the primitive language; the Anglo-Saxon retained five of them; we have saved but one of the oblique cases, the genitive, (our "possessive.") Prepositions, adverbial prefixes to the verb, of mixed pronominal and verbal origin, were from a very early time important aids in directing and limiting the action expressed by the verb; these only later, and by degrees, detached themselves from the verb, and came to belong to the noun, assuming the office of its disappearing case-endings. The article is the part of speech of most modern origin, the definite article growing out of the demonstrative pronoun, the indefinite out of the numeral *one*.

At what rate these processes of growth went on at the beginning, how rapid was the development out of monosyllabic barrenness into the wealth and fertility of inflective speech, we can never hope to know. The conditions of that ancient period, and the degree in which they could quicken the now sluggish processes of word-combination and formation, are beyond our ken. We know only that, before the separation of the Indo-European tribe into the branches which later became the nations of Europe and southwestern Asia, so much of this linguistic development had taken place that its traces remain uneffaced, even to the present day, in the languages of them all; and, also, that the work was accomplished hundreds of years, if not thousands, before the light of recorded history breaks upon the very oldest member of the family.

Much of what has been shown to be true of the history of Indo-European language is true also of that of other divisions of the human race. All the varied forms of speech which fill the earth have grown into their present shape by development out of such simple elements as we have called roots; roots, too, have been everywhere of the same two classes, pronominal and verbal, and the earliest forms have been produced especially by the combination of the two. Linguistic families are made up of those languages which have recognizably descended, in the ordinary course of linguistic tradition, from a common ancestor. But these great families are found to differ from one another, not only in their material, but also in their management of it; in their

apprehension of the grammatical relations to be expressed by the combination of elements, and in the general way in which they apply their resources to the expression of these relations. Indo-European languages are what is generally called "inflective." By this is meant, that they show a peculiar aptitude in closely combining the radical and formal elements, forgetting their separate individuality, and accepting the compound as integral sign of the thing indicated; submitting it then, as a whole, to the altering processes of linguistic growth. This tendency shows itself very differently in different constituents of the language: in *untruthfully*, for example, the four elements are held independently apart; while in *sing*, *sang*, *sung*, *song*, inflection has reached its extreme result, substituting an internal variation for original aggregation. The value of this distinction will appear more clearly as we go on to consider the characteristics of the other great families. We will take them up in an order partly geographical, partly based upon their relative importance.

The second family is the Semitic, or Shemitic, so called because the descent of most of the nations speaking its languages is traced in the Bible to Shem. Its principal branches are: 1. The northern, Syriac or Aramaic. 2. The central, Hebrew and Phenician. 3. The southern, Arabic, with its outliers in Eastern Africa, the languages of Abyssinia. It is a strongly marked group, and, though occupying but a narrow territory, is of prime consequence, from the conspicuous part which the race speaking it has played in the history of the world. In the great empires of Mesopotamia the Semitic race first rose to high importance; then in the commercial and civilizing activity of the Phenicians, whose colony, Carthage, long disputed the dominion of the world with Rome. Meantime, the politically almost insignificant little people of the Hebrews were producing a religion and religious literature, which, made universal by Christ, were to become the mightiest elements in history. Finally, in the Mohammedan uprising, the third branch of the race advanced suddenly to a leading place, and for a while threatened even to reduce to vassalage the Indo-European nations; and it is still a conquering and civilizing power in parts of Asia and Africa.

The Semitic type of language is also inflective, like the Indo-European, but not in such a way as implies any historical connexion between the two. The Semitic tongues are in many respects of a more strange and isolated character than any others known. Their most fundamental peculiarity is the triliterality of their roots, every Semitic verbal root containing just three consonants. And it is composed only of consonants: their vocalization is almost solely a means of grammatical flexion. Thus, *q-t-l* is a root conveying the idea of "killing;" then *qatala* means "he killed;" *qutila*, "he was killed;" *uqtul*, "kill;" *qâtil*, "killing;" *iqtâl*, "causing to kill;" *qatl*, "murder;" *qitl*, "enemy;" *qutl*, "murderous;" and so on. Prefixes and suffixes are also used, but to only a limited extent; there is little left for them to do; the formation of derivative from derivative, by accumulation of affixes, is almost totally unknown. This significant vocalization is, to our knowledge, an ultimate fact in Semitic speech in all its forms, as is the radical triliterality; but it seems impossible to regard the latter, especially, as absolutely original; and many attempts are made, with but indifferent success as yet, to reduce the roots to a simpler and less Procrustean form, out of which they should be a development. The different languages are of very near relationship, like German, Dutch, and Swedish, rather than like German, French, and Russian, for instance. Nor have they varied in the course of their recorded history to anything like the same extent with the Indo-European languages. Everything in Semitic speech wears an aspect of peculiar rigidity.

The Semitic verb is strikingly unlike ours in its apprehension of the element of time. It distinguishes only two tenses, whose chief distinction is that of complete and incomplete action: each may be, in different circumstances, either

past, present, or future. Of wealth of modal forms there is but little; distinctions of the action of transitive, causal, intensive, iterative, reflexive, and the like, by so-called conjugations, are multiplied instead. In their nouns, the Semites distinguish two genders, masculine and feminine, and three numbers; but cases are almost wanting, only the Arabic separating nominative, genitive, and accusative. The substantive verb is mostly wanting. The language is poor in particles and connectives; sentences are strung together, not interwoven into a period. The characteristic stiffness is also shown in the development of signification. Words applied to intellectual and moral uses remain metaphors; the figure shows through, and cannot be lost sight of. Semitic speech, then, is rather pictorial, forcible, vivid, than adapted to calm and reasoning philosophy.

The next family of languages is one of much greater extent and variety. It covers the whole northern portion of the eastern continent, with most of Central Asia, and parts of both Asia and Europe lying further south. We will call it the Scythian family; it is known also by several other names, as Ural-Altaic, Tataric, Mongolian, Turanian. It is divided into five principal branches: 1. The Ugrian, or Finno-Hungarian, which is chiefly European in situation, including the languages of the Lapps, the Finns, and the Hungarians, with their congeners in the Russian territories, on both sides of the Ural. 2. The Samoiedic, in Siberia, of small consequence. 3. The Turkish, or Tataric, spoken by races who have played some conspicuous part in modern history, especially in the dismemberment of the Mohammedan empire: its subdivisions are numerous, and extend from Turkey in Europe to the lower Lena, in Northern Siberia. 4. The Mongolian, the language of a people who in the 13th century overwhelmed nearly all the monarchies of Europe, and established for a brief period an empire the widest the world has ever seen: the Mongols now live in insignificance under Chinese domination. 5. The Tungusic, in the extreme east, having for its principal branch the Manchu, spoken by the present ruling dynasty and tribe in China.

The Scythian races have played but a subordinate part in human affairs. War and devastation have been their chief trade: they have shown no aptitude for advancing civilization, and but little for appropriating it. No written monuments of their languages carry us back to a past at all remote. But it is claimed of late by students of the Assyrian and Babylonian inscriptions, that one of their languages is a Scythian dialect, of the Finno-Hungarian branch, and even that those who spoke it were the founders of the civilization of that region. If this is established as true, it will greatly modify the aspect of ancient ethnological history.

The linguistic tie which binds together the branches of this great family is but a weak one, much less unequivocal than in the other families we have noted. There is less correspondence between them in linguistic material and forms; either their separation is very remote, or they have had a peculiarly mobile and alterable structure. Their chief resemblances are of morphological character; they are all alike "agglutinative;" the combinations by which their words are formed are of a loose nature; the root or theme is held apart from the suffixes, and these from one another, with a distinctive consciousness of their separate individuality. All formative elements follow the root to which they are attached; prefixes are unused; the root, which is monosyllabic, remaining pure and unchanged, whatever accretions it may receive. It, however, usually affects the suffixes, in a manner which constitutes one of the striking phonetic peculiarities of the family. The vowels are divided into two classes, heavy and light, and only vowels of the same class are allowed to occur within the limits of the same word; hence, the vowels of all suffixes are assimilated to that of the root. Thus, in Turkish, from *bâbâ* comes *bâbâ-lar-um-dan*, "from our fathers;" while from *dedeh* comes *dede-ler-in-den*, "from their grandfathers." This is usually called

the "law of harmonic sequence of vowels." Varieties and irregularities of conjugation and declension are almost wholly wanting in Scythian grammar.

The rank of the Scythian languages in the general scale of human speech, notwithstanding their euphonious structure and great wealth of forms in certain departments, is but an inferior one. Those of the western or European branch are decidedly the noblest, and they diminish in value eastward, the Tungusic being the poorest of all.

There are those who would give the Scythian family a yet wider extension, even making it include most of the other Asiatic tongues, with those of the islands. Such sweeping classification, in the present state of our knowledge, has no scientific value, and is even opposed to the plainest evidences of linguistic structure and material. One group, that of the Tamulic or Dravidian dialects of Southern India, is most confidently, and with most plausibility, claimed as Scythian, and may probably yet be proved such.

China and Farther India are occupied by races whose languages form a single class. Their distinction is that they are monosyllabic; they have never grown out of that original stage in which, as we have seen, Indo-European speech also had its beginning. Their words are still roots, of indeterminate logical form; they are made parts of speech only by the consenting apprehension of speaker and hearer, guided by their order and by the general requirements of the sense. But while the different languages of the class agree in general morphological character, they show great diversity in material, and the nature and degree of their relationship is very obscure. The Chinese is infinitely the most important among them. Its abundant literature goes back even into the second thousand years before Christ. It has only about 450 different phonetic combinations in its vocabulary; which, however, by change in the tone of utterance, are made into rather more than twice that number of distinct words. Yet this scanty apparatus, by the power which the mind has over its instrument, has been the means of expression of far higher, profounder, and more varied thought, than the majority of highly organized dialects spoken among men. China has been the mother of culture to the races lying south, east, and west of her territory: the rest of the world she has affected mainly through the products of her ingenuity and industry.

Those who speak the Malay-Polynesian languages fill all the islands, from the coast of Asia southward and eastward, from Madagascar to the Sandwich group, from New Zealand to Formosa. Only the present spoken dialects are known, and most of those but very imperfectly, so that their groupings and degrees of relationship are little understood: there may prove to be more than one distinct family among them. Their phonetic form is of the simplest kind. Their roots are prevailingly dissyllabic in form, and of nominal rather than verbal meaning. Reduplication is a common mode of their development; the rest is accomplished more by prefixes than suffixes. Anything that can properly be called a verbal form is hardly to be found in most of the dialects; mood, tense, number, gender, case, are wanting.

The oldest dated monuments of ancient culture, the oldest written records, are found in the valley of the Nile. The earliest form of Egyptian speech is preserved on tables of stone and rolls of papyrus held by dead hands; a later, the Coptic, has a Christian literature of the first centuries after Christ, but the Coptic also has been extinct now for more than two centuries. It was of the simplest structure; its monosyllabic roots had value as verbs and as nouns, and only primary derivatives were formed from them; nor were its suffixes, for the most part, more closely attached than those of the Scythian family. In some of its constructions it was as bald as the Chinese, and even more ambiguous. It agrees with the Indo-European and Semitic languages in distinguishing gender in its forms; no other human languages do this. There are apparent signs of relationship between Egyptian and Semitic which lead many

scholars to entertain the confident opinion that the two descend from a common ancestor; this, however, is as yet by no means to be regarded as certain. Many of the tongues of Northern Africa, and the Hottentot and Bushman, in South Africa, are also asserted to exhibit signs of an ultimate connexion with Egyptian. Excepting those dialects which are either clearly Semitic, or claimed to be of kindred with Semitic or Egyptian, Africa is filled with a great variety of tongues, forming a distinct family. They are, in a certain way, rich in forms, and have some striking and peculiar traits. The use of preformatives characterizes them; a root never appears without a prefix of some kind, and the prefixes are varied to accord with that of the dominant word in the sentence, producing a kind of syntactical alliteration.

There remains for consideration, of the great families of human speech, only that one which occupies the American continent. It is too vast and varied to be dealt with here in any detail. Isolation of communities and the consequent indefinite separation into dialects have been carried in America to an extreme. Moreover, there is a peculiar changeableness of material, hard to explain and account for, which causes that two branches of a tribe which have been separated but a brief time speak languages which are mutually unintelligible, and of which it is even hard to trace the relationship. But it is believed that a fundamental unity lies at the base of all the infinite variety of American dialects, from the Arctic Circle to Cape Horn; whatever their differences of material, there is a single type or plan on which their forms are developed and their constructions made. It is called the incorporative, or polysynthetic. It tends to the aggregation of the parts of the sentence into one great word; to the substitution of an intricate compound for the phrase with its separated and balanced numbers.

No linguistic evidence of any real value has yet been adduced going to show the affinity of American with Asiatic language, nor has the time yet come for a fruitful discussion of the question. To make a bare and immediate comparison of the modern dialects of the two continents is altogether futile. When the comparative philology of the separate families is fully worked out, from the collation and analysis of all attainable material in each, if we shall find ourselves in a position to judge and decide the question of Asiatic derivation, we shall have reason to rejoice at it. What we have to do at present is simply to learn all that we possibly can about the aboriginal languages of this continent; our national honor and duty are peculiarly concerned in the work, toward which, with too much reason, European scholars accuse us of indifference and inefficiency. The Smithsonian Institution has recently taken up the subject, under special advantages and with laudable zeal, and all Americans should countenance and assist its efforts by every means in their power.

Before closing this cursory and imperfect review of the great families of human language, we should glance at one or two isolated languages or groups, hitherto unclassified. One of the most noteworthy is the Basque, spoken on the borders of France and Spain by the representatives of the ancient Iberians, and perhaps the scanty relic of a race earlier than the irruptions of the Scythian and Indo-European tribes. Another is the Etruscan, of Italy, saved in scanty inscriptions, which offer an unsolved and probably insoluble problem to the linguistic student. In the Caucasian mountains, again, appears a little knot of idioms which have defied the efforts of scholars to connect them with other known forms of speech. Each family has, as may be seen even from our hasty sketch, its own peculiar characteristics, which distinguish it from every other. By such sweeping classifications of them as into monosyllabic and polysyllabic, into isolating, agglutinative, and inflectional, or the like, little or nothing is gained. True classification must be founded on a consideration

of the whole complicate structure of the languages classified; it must, above all, be historical, holding together, and apart from others, those groups which give evidence of genetic derivation from a common original.

On reviewing this division of the families of language, any one will be struck by its non-agreement with the divisions based on physical characteristics. This brings up the important question as to the comparative value of linguistic and physical evidence of race. A reconciliation of their seeming discordance must be sought and finally found, for the naturalist and linguist are both trying to work out the same problem—the actual genealogical history of human races—and they cannot disregard each other's results. Their harmonious agreement can only be the result of the greatly advanced and perfected methods and conclusions of both. Nothing more can be attempted here than to note certain general considerations bearing upon the subject.

In the first place, language is no certain evidence of descent. As was shown in the first lecture, language is not inherited, but learned, and often from teachers of other blood than the learner. Nor does mixture of language prove mixture of race. The Latin part of our vocabulary was brought us by men of Germanic descent, who learned it from Celts and Germans, and they from a mixed mass of Italians. These defects of linguistic evidence have always to be borne in mind by those who are drawing conclusions in linguistic ethnology. But their effect must not be exaggerated; nor must it be overlooked that physical evidence has quite as important defects. The kind and amount of modification which external circumstances can introduce into a race-type is as yet undetermined. Many eminent naturalists are not unwilling to allow that all existing differences among men may be the effect of processes of variation, and that the hypothesis of different origins is at least unnecessary. Hence, as a race may change its language, and not its physical type, it may also do the contrary. Language may retain traces of mixture undiscoverable otherwise. Language may more readily and surely than physiology distinguish mixed from transitional types. In many respects linguistic evidence has a greatly superior practical value; differences of language are much the more easily apprehended, described, and recorded. Individual differences, often obscuring race-differences of a physical characteı, disappear in language. Testimony coming down from remote times is much more accessible and authenticable in language. Discord between the two, or question as to relative rank, there is none, or ought to be none. Both are equally legitimate and necessary modes of approaching the solution of the same difficult and, in its details, insoluble problem, man's origin and history. Each has its notable limitations, and needs all the aid it can get from the other and from recorded history to supply its defects and control its conclusions. But the part which language has to perform in constructing the ethnological history of the race must be much the greater. In laying down grand outlines, in settling ultimate questions, the authority of physiology may be superior; but the filling up of details, and the conversion of a barren classification into a history, must be mainly accomplished by linguistic science.

Another important question is, what has the study of language to say respecting the unity of the human race? This question can already be pretty confidently answered, but the answer must be a negative one only. Linguistic science can never hope to give any authoritative decision upon the subject. To show that it can never pretend to prove the ultimate variety of human races is very easy. It regards language as something which has grown by degrees out of scanty rudiments. It cannot assume that these rudiments were produced by any other agency than that which made their after combinations. It cannot say how long a time may have been occupied in the formation of roots, or how long the monosyllabic stage may have lasted; and it must confess it altogether

possible that an original human race should have separated into tribes before the formation of any language so distinctly developed, and of such fixed forms, as should leave traceable fragments in the later dialects of the sundered portions. Among all the varieties of human speech there are no differences which are not fully explainable upon the hypothesis of unity of descent.

That the linguistic student also cannot bear positive testimony in favor of such descent is equally demonstrable, although not by so direct an argument. There is here no theoretic impediment in the way, but a practical one. It might be hoped that traces of an original unity would be discoverable in all parts of human language; only examination could show that such is not the case. But investigation, however incomplete, has already gone far enough to leave no reasonable expectation of making the discovery.

The processes of linguistic change alter the constituent parts of language in every manner and to every degree, producing not only utter difference between words which were originally one, but also apparent correspondence between those which are radically unconnected. There are no two languages on the face of the earth between which a diligent search may not bring to light resemblances which are easily proved by a little historical study to be no signs of relationship, but only the result of accident. Now, the more remote the time of separation of two related languages, the more numerous will be their differences, the more scanty their resemblances; hence, the more ambiguous will be the indications of their connexion; until finally a point is reached where it is impossible to decide whether apparent coincidences which we discover are genuine, or only accidental, and evidence of nothing; and, in the comparison of languages, that point is actually reached. When we come to hold together the forms of speech belonging to different families, the evidence fails us. It is no longer of force to prove anything to our satisfaction. The families are composed of such languages as can be seen to have grown together out of the radical stage. If there is community between them, it must lie in their roots alone; and to give the comparison this form is virtually to abandon it as hopeless. To trace out the roots of any family, in their ultimate form and primitive signification, is a task of the very gravest difficulty. By the help of the great variety and antiquity of its dialects, and especially by the Sanscrit, the task can be somewhat satisfactorily accomplished for the Indo-European tongue; but the Semitic roots, as already explained, are of the most perplexingly developed form. Radical correspondences among the great branches of the Scythian family are hardly sufficient to prove the ultimate relationship of those branches; and to hope that, in the blind confusion of Malay, African, and American dialects, linguistic analysis will ever arrive at a confident recognition of their primitive germs, is altogether futile. Accidental correspondences are, if anything, more likely to appear among roots than in the forms of developed speech. Authorities are much divided upon the question whether the Indo-European and Semitic families are proved connected, with a decided preponderance of the best and safest opinions on the negative side. If it may possibly be hoped that their connexion will yet be established, with the help of evidence coming from outside of language, the same hope cannot be entertained as to the connexion of either of these with any other family, and yet less as to the inter-connexion of all the families.

We come, finally, to consider the origin of language. We may claim that the problem has been greatly simplified by what has already been proved as to the history of speech. Did we find the latter everywhere and always a completely developed and complicated apparatus, we might be tempted to despair of explaining its origin otherwise than by the simple hypothesis of a miraculous agency. But we have seen that the wealth of the noblest tongues comes by slow accumulation from an early poverty. We have only to satisfy ourselves how men should have become possessed, at first, of the scanty and humble germs of language. And, in the first place, there is no reason for supposing them

generated by any other agency than that which is active in their after combination and development; namely, by the conscious exertion of man's natural powers, by his use of the faculties conferred upon him for the satisfaction of the necessities implanted in him. In this way, and in no other, is language a divine gift. It is divine in the sense that man's nature, with all its capacities and acquirements, is a divine creation. It is human, in that it is a product of that nature, in its normal workings.

It is highly important that we make clear to ourselves what is the directly mpelling force to the production of language. It is not any internal and necessary impulse to expression on the part of thought itself, although this is very often maintained; it is the desire of communication. One man alone would never form a language. Two children could not grow up together without acquiring some means of exchange of thought. Language is not thought, nor thought language; nor is there a mysterious and indissoluble connexion between the two, so that we cannot conceive of the existence of the one apart from the other. But thought would be awkward, feeble, and indistinct, without the working apparatus afforded it in language. The mind, deprived of such an instrument, would be, as it were, lamed and palsied. The possession of ideas, cognitions, reasonings, deductions, imaginings, hopes, cannot be denied to the deaf and dumb, even when untaught any substitute for spoken language; nor, indeed, even to the lower animals, in greatly inferior and greatly varying degree. Thought is anterior to language and independent of it. It does not require expression in order to be thought. The incalculable advantage which it derives from its command of speech, though a necessary implication in the gift of speech to man, comes incidentally, growing out of that communication which man must and will have with his fellow. A word, then, is not a thought; it is the sign of thought, arbitrarily selected and conventionally agreed upon. It is the fashion to cry down the use of the word conventional as applied to language; but, rightly understood, it precisely expresses the fact. It does not imply the holding of a convention and formal discussion, but the acceptance and adoption into use, on the part of a community, of something proposed by an individual; and in no other way, as has been shown above, does anything in language originate; nor did it, back to the very beginning. Every root-syllable was first used in its peculiar sense by some one, and became language by the assent of others.

These considerations relieve the remaining part of our problem of much of its difficulty. Under the outward impulse to communication, thought tends irresistibly toward expression: it will have expression, and, were it destitute of articulate speech, it would have sought and found other means—gestures, attitudes, looks, written signs, any or all of these. But the voice was the appointed and provided means of supplying this great want, and no race of men, accordingly, is found unprovided with articulate speech. It remains to inquire how men should have discovered what the voice was meant for, and have applied it to its proper use. Several theories have been proposed in explanation of this. One, the onomatopoetic, supposes that the first names of objects and acts were generated by imitation of the cries of animals and the noises of dead nature; another, the interjectional, regards the natural sounds which we utter when in a state of excited feeling, our exclamations, as the beginnings of speech; another compares man's utterance with the ringing of natural substances when struck, and holds that man has an instinctive faculty for giving expression to the rational conceptions of his mind. The last of these is believed to be destitute of all value, as grounded in unsound theory, and supported by nothing in our experience or observation. The other two are so far true that it must be granted that exclamations and imitated sounds helped men to realize that they had in their voices that which was capable of being applied to express the movements of their spirits. But the study of language brings to light no interjectional

roots; and onomatopoetic ones, although sometimes met with, are rare, at least in the better known families of language, and in great part of late formation. Evidence does not show, and theory does not require, that the actual beginnings of speech should have been of either character. The process of root-making was in much the greatest part a free and arbitrary one; it was, as we may with especial propriety call it, a tentative process, a devisal and experimental proposal of signs, to be thenceforth associated by a community with conceptions which pressed for representation. Objective and absolute connexion between sound and sense there was none, except in words of onomatopoetic formation; of a subjective connexion, a guiding analogy, we do catch occasional glimpses, or seem to catch them; they are too subtle and evanescent to be believed in with confidence, nor have we ground for suspecting their wide occurrence. There is thus enough of obscurity, of uncertainty, resting upon the earliest period of linguistic growth; but of mystery, hardly any; the process is not beyond our ken, although its details are out of our knowledge.

Of all animals, man is the only one that has proved himself capable of originating a language. For this, the general reason, that man's endowments are vastly higher than those of the inferior races, is the best that can be given. When philosophers shall have determined precisely wherein lies man's superiority, they will at the same time have explained his exclusive possession of speech. If, however, it were necessary to say in what mode of action lay that deficiency of power in the lower animals which, more than any other, put language out of their reach, we should incline to maintain that it was the power of distinct reflection on the facts of consciousness; of analyzing impressions, and setting their parts so clearly before the internal sense as to perceive that each is capable of a distinct sign. Many animals come so near to a capacity for language as to be able to understand and be directed by it, when addressed to them by man; nor is their condition without analogy with that of very young children, whose power of comprehending language is developed much earlier and more rapidly than their power of employing it. It may well be questioned whether, as regards capacity for speech, the distance from the unimpressible oyster, for instance, to the intelligent dog, is not vastly greater than that from the dog to the lowest and least cultivable races of men.

MEMOIR OF C. F. BEAUTEMPS-BEAUPRÉ.

BY M. ELIE DE BEAUMONT,

ERPETUAL SECRETARY OF THE FRENCH ACADEMY OF SCIENCES.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

To this Academy no species of scientific renown is alien; and if such men as la Pérouse, d'Entrecasteaux, Baudin, Dumont d'Urville, have disappeared from the stage of the world without having been numbered in its ranks, it was because an inauspicious destiny arrested their career. Their place here was already marked. To have obtained it would have been to them, next to the consciousness of duty fulfilled, the highest of gratifications. To you, gentlemen, the privilege of crowning their memorable labors by your suffrages would have been a subject of the most just self-congratulation. Those labors death, which has snatched away their authors, has not withdrawn from your domain. It is still grateful to you to extol them, and your committee has concurred with me in thinking that I could prefer no better claim to your favorable attention than by attempting to retrace, on this occasion, the life of a colleague who knew how to obtain and to justify all your sympathies, and whose name invariably recalls those of the heroes of hydrography we have named, of whom he was, with better fortunes and not less daring, the companion, the rival, or the master.

Charles-François Beautemps-Beaupré was born August 6, 1766, at Neuville au Pont, a village situated one league north of Sainte Menehold, in that part of Champagne which now forms the department of the Marne. His father was an unpretending tiller of the soil, and the young François, who seemed destined to cultivate, in his turn, the rather prosaic fields of that worthy country, passed his first years in youthful sports on the pleasant hills which, branching from the Argonne, agreeably diversify the banks of the Aisne. His constitution, naturally robust, and strengthened by country exercise, received on one occasion a severe shock. While heedlessly playing with the rope of the parochial bell he fell with violence, and sustained such injuries of the head as to make trepanning necessary. The operation was no doubt skilfully performed, for the young sufferer became, with advancing years, a man of tall stature, of a noble and expressive mien, and retained, for nearly eighty years, the use of the exalted faculties which won him a place in this assemblage. I have not been able to recover the name of the modest provincial surgeon to whom, under Providence, our colleague was indebted for life and intelligence, and who, perhaps, never knew the full value of the head he had been instrumental in restoring.

M. Beautemps-Beaupré passed, indeed, only the years of childhood at his native village. Among his relations was an eminent geographer, M. Jean Nicolas Buache, the head of a geographical establishment derived by collateral inheritance from the family of Delisle—a family wholly devoted to science, and known, through more than a century, for its connexion with almost every publication relating to geography, astronomy, and the marine. M. Buache, visit-

ing Neuville au Pont about the year 1776, was struck with the intelligent countenance of his young relative. He was pleased at the idea of associating with himself a docile intelligence which might be trained to the conduct of the patrimonial business, and readily induced the little Beaupré to accompany him to Paris. Thus the latter found himself installed, at the age of ten years, in the midst of the hereditary traditions of a house which had become, in some sort, the focus of geographical studies. He was charged with the arrangement and preservation of those charts, atlases, and globes with which we have most of us been occupied at some period of our lives. To this labor, which would have repelled the generality of young persons, he gave himself with unbounded devotion. He lived among his dear maps, assorting, adjusting, studying them; hence he was not long in mastering all that was necessary for understanding them. His vocation stood revealed to him; nor, with such innate tastes, could his eventual accession to this Academy be a matter of doubt, provided that for him, also, the condition stipulated in the distich of La Fontaine should be realized:

> "Little fish to large will grow,
> If God shall only life bestow."

M. Buache, gratified at the manifestation of so happy a turn, afforded every facility in his power for its development.

The attention of this learned geographer was by no means confined to the commerce of his establishment. He had assisted in the education of the three princes who became, successively, Louis XVI, Louis XVIII, and Charles X, and maintained with the first of these monarchs, himself a distinguished geographer, relations of confidence founded on a similarity of tastes and studies. It is to be presumed that he contributed much towards shaping the views of the excellent King in relation to the expedition of la Perouse, and being intrusted, jointly with M. Fleurieu, with the preparation of instructions for the voyage—instructions strongly impressed with the benevolent spirit of Louis XVI—it became necessary for him to execute in the short space of three months a numerous series of charts. Naturally he turned for assistance in this labor to his young coadjutor, with whose talent for this species of design he had been so much delighted; and, quite as naturally, the youthful enthusiast, in whom there was much more than the material for a draughtsman, grew enamored, as he proceeded, not only of the charts but of the expedition, and eagerly pressed to be allowed to embark on one of the frigates. Happily for himself and for science, M. Buache decided that, at the age of eighteen, there was yet too much for him to learn to make it advisable that he should engage in such an enterprise, and thus prevented his taking part in that fatal expedition from which no one was destined ever to return.

The young Beaupré had not, however, escaped the notice of M. de Fleurieu, and was transferred as engineer in 1785 from the department of the Marine, in which he had heretofore served under the orders of M. Buache, to that of the Controls, where, in immediate subordination to M. de Fleurieu, he was required to assist in the execution of the charts of the Baltic *Neptune*.

Meanwhile the expedition commanded by la Perouse had sailed from Brest, August 1, 1785. After having traversed the coasts of the Pacific ocean in all directions, and moored in the harbor of Botany Bay, it had again put to sea, March 10, 1788, in order to prosecute the route marked in its instructions. From that time nothing had been heard of it, and apprehensions for its safety began to be entertained which were unhappily too well founded.

The National Assembly having petitioned the King to despatch armed vessels in search of the distinguished navigator, two new frigates, *la Recherche* and *l'Esperance*, were designated to sail, under the orders of Rear-Admiral Bruny d'Entrecasteaux, upon this laudable mission; and this time M. Beau-

temps-Beaupré obtained the favor of accompanying the expedition. He was assigned, July 31, 1791, under the title of first hydrographical engineer, to the frigate *la Recherche*, commanded by the admiral in person, and reported himself at Brest, whither he had repaired in company with M. de la Billardiere, the botanist of the expedition, and destined himself also to become a member of this Academy.

The two vessels sailed September 29, 1791, at which time Beaupré was twenty-five years of age. By his labors during six years in the compilation of the *Neptune of the Baltic sea*, he had thus early become an experienced chartographer, and the expedition now departing offered the happiest occasion for the application of his talents in this line; for the admiral, being about to explore with great minuteness all the coasts where traces of la Perouse might be expected to be found, had received orders to determine at the same time their hydrography with all possible compactness.

After having doubled the Cape of Good Hope, the expedition passed in sight of the isle of Amsterdam, coasted at a distance the southern shores of New Holland, and came to anchor towards the southeast point of Van Diemen's Land, at the then desert entrance of the river on which now stands the city of Hobarttown. It next penetrated into the Pacific ocean, followed the western coast of New Caledonia and the northern of New Guinea, passed to the northwest of Amboyna and Timor, to the west of New Holland, explored in detail the south coast of that vast region, and, after having thus made its entire circuit, again cast anchor, January 21, 1793, in the south part of Van Diemen's Land.

Having completed, during the finest month of the austral summer, important hydrographical labors commenced the previous year, and particularly the survey of the straits of d'Entrecasteaux, which separate the isle of Bruny from the main land, the expedition again sailed, February 27, and passed anew into the wide Pacific. Directing its course towards all the points where la Perouse could be supposed to have touched or to have been driven, after his departure from Botany Bay five years before, the expedition visited Tongataboo, one of the Friendly islands, and once more shaped its course towards New Caledonia, which was now reached from the northwest. Some idea of the incidents and perils of these courses may be conveyed by a few passages of the admiral's narrative: "On the eve of our arrival at New Caledonia, April 17, 1793, it blew a hard gale; the atmosphere was thick, but not so dark as to induce me to lose a night off the Cape. I gave orders to proceed under easy sail. About three in the morning it grew very dark, and the cries of numerous birds were heard near the frigate, an almost certain indication at that hour, of the neighborhood of land. Although day was not far off, M. Merite, officer of the watch, prudently decided to bring to, and scarcely had objects become distinguishable, when a low coast presented itself to view; an instant after it was discovered to be surrounded with breakers on which we should certainly have struck but for the precaution just mentioned; for we had been making two leagues an hour under topsails alone, closely reefed. This dangerous ledge was reconnoitred, and a special draught of it carefully executed. Its length from north to south is from nine to eleven miles, and its breadth, east and west, seven to eight. We saw to the east of this reef two small wooded islands, with a third larger midway between them: these we have named the *Beaupré islands*."*

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* The claims of M. Beautemps-Beaupré to a distinction of this kind were incidentally recognized by the distinguished and lamented explorer, Sir John Franklin. Being on a visit to Paris, just before his departure on the expedition which was destined to so fatal a result, he called on M. Beautemps-Beaupré, and, speaking of Van Diemen's Land, of which Sir John had been governor, learned from the lips of our colleague that the latter had been the first explorer of the site on which now rises Hobarttown, the capital of the island. "How

When his name was thus conferred, M. Beautemps-Beaupré had been daily prosecuting his labors for more than twenty months under the eyes of the admiral and his officers, and the testimonial may, therefore, be regarded as the more deliberate and honorable.

"The same day," continues the admiral, "at half after 1 o'clock, we descried New Caledonia, and in two hours were a mile distant from the reef on the eastern coast of this great island, which seemed to be bordered by it, as the western coast had been ascertained to be in 1792. * * * * As the entrance of the harbor of Balade, where I proposed to come to anchor, was only marked by an interruption of the reef which borders the coast, we followed this reef closely in order not to miss the opening. We reached the pass by 2 o'clock, and a favorable tack gave us hopes of gaining the anchorage, when it was signalled that the other frigate, *l'Esperance*, had struck."

Happily the imperilled vessel was safely extricated, and the two frigates finally cast anchor very nearly at the spot where Captain Cook had done in 1774.

"The naturalists of the expedition repaired, April 25, to the neighboring mountains, and M. Beaupré ascended with them in hopes of discovering the reefs with which the channels of Balade are bestrown, and of fixing their position. The sea was discernible to the east, west, and north, and the isles of Balabra, Reconnaissance, and many other points which had been entered in the maps of 1792 were recognized. The positions of these were determined by M. Beaupré with reference to the observatory of Balade, with the view of connecting the trigonometrical operations of this year with those of the preceding one. From the top of these mountains the shelf which borders the other side of New Caledonia was perceived, and an interruption distinguished, which, after renewed observations, seemed to correspond with that discovered the previous year in visiting the western coast."

The expedition left the roads of Balade May 9, 1793, and soon after encountered the dangerous reefs which stretch to the NW. of New Caledonia; these having been examined but imperfectly by Cook, have received the name of the *reefs of d'Entrecasteaux*. Twice, at the break of day, were the ships of the last-named navigator found to have so closely approached this barrier, that there was barely room for the evolution by which they were extricated. Directing his course northeastwardly towards the island of Santa Cruz, the admiral gave the name of *la Recherche* to an island in the vicinity of the former, whose latitude and longitude were determined to be, within but a few minutes, 11° 40′ south, and 164° 25′ east. During the numerous courses made by the vessels in the archipelago of Santa Cruz, M. Beautemps-Beaupré, favored by fine weather, succeeded in fixing the position of a multitude of points, as well on the principal island as its accessories.

According to the method which he had adopted for making his observations, and which has since become of general use, he first made at each station a draught of the coast, in which he indicated by letters or numbers not only the most remarkable objects, but wrote the measures of the angles observed, the bearings of the points with respect to one another, the estimate of distances, &c. The draughts, on which were to be written the results of the observations made

much do I regret," exclaimed Sir John, "that I was ignorant of the circumstance! I should have bestowed your name on the finest portion of the city."

Captain Flinders, who, in 1801–1803, conducted an expedition "for the purpose of completing the discovery of that vast country" to which he gave the name of *Terra Australis*, (afterwards changed to Australia,) and who published an account of his voyage in two 4to. volumes, accompanied by an atlas, bears testimony, as well in notes engraved upon the maps as in passages of the text, to the accuracy of the labors of our colleague. In the introduction to the work it is said: "The charts of the bays, ports, and arms of the sea at the southeast end of Van Diemen's Land, constructed in this expedition by M. Beautemps-Beaupré and assistants, appear to combine scientific accuracy and minuteness of detail, with an uncommon degree of neatness in the execution. They contain some of the finest specimens of marine surveying, perhaps, ever made in a new country."

on board, could not be taken with too much rapidity, for it was necessary that the ship should not materially change its place during the time of the operation. The principal operations which serve as a foundation for the charts constructed by M. Beaupré are such as were executed either at midday, or simultaneously with the observations of horary angles; that is to say, at such times of each day as the position of the vessel was determined by astronomical observations and the chronometer. On these occasions he assembled around him the greatest possible number of observers, and he had found or formed a great many among the officers of the frigate. Just *one minute* before taking the observations he made a sketch of the coast under view, beginning with those parts of it which, being most remote, would undergo least change of outline by reason of the movement of the ship; then, precisely at the moment when the astronomical observations were taken, he measured the angular distance between the object which he had designated to his assistants as the point of departure and one of the remarkable places of the coast, while each of the assistants measured the angular distance of the same point of departure from one of the other objects embraced in the survey. The results of these simultaneous observations were afterwards transferred to the sketch which had been made of the outline of the land. All the angular measures were taken with *Borda's repeating circle.*

When the sun was not too high above the horizon, one of the observers measured the distance of that body from one of the remarkable points of the coast; by means of the heights of the sun observed at the same moment by M. de Rossel, and from the distance measured, M. Beaupré obtained the astronomical bearing of that point, whence he deduced the bearing of all the points between which angles had been taken.

Two compasses were always directed, during the observations, on the place chosen as a point of departure for the angles, and the mean of the bearings given by those instruments was transcribed in the collection of notes, and this whether an astronomical bearing had been obtained or not. In the first case the magnetic indication served to show the variation of the needle, and in the second to supply, though imperfectly, the absence of an astronomical observation. If circumstances, which, however, occurred but rarely, prevented the co-operation of a sufficient number of observers to take simultaneously the angles of all the remarkable points necessary to be determined, M. Beaupré arranged several circles of reflection, so that each observer might promptly take two or three angles, without being obliged to write them on the spot; and these observations, made with a rapidity proportionate to the expertness of the observer, were found to agree almost as exactly as those made simultaneously.

M. Beaupré, who drew the chart with as much facility as exactness, found a marked advantage in embodying the results observed as promptly as possible, for he had then all the circumstances of the observations present to his mind. It was not seldom that he was enabled in this way to detect and remedy inadvertencies committed in writing the angles measured. The precision of his graphic constructions ever rendered it practicable for him to verify, and sometimes to correct, with great probability, the positions of the ship, determined several times a day by astronomical observations, combined with the indications of chronometers and the estimate of courses.

The means of verification resulted, in part, from the fact that the observations of each station gave him a series of visual lines, springing essentially from the same point, and forming known angles, whether with one another or with the astronomic meridian, or at least with the magnetic meridian, itself determined by an observation made at nearly the same time. They resulted, moreover, from the circumstance that all the visual lines directed from different stations on the same object, such as a cape or a mountain, must, on the draught, intersect one another at the representation of that object. When, at the first trial, these did

not meet, a series of approximations tending to modify in an admissible degree the position of the ship at the different stations sufficed to establish the necessary junction. The approximations in question might be made with still more rigor by calculation, and one of our most scientific hydrographers, M. de Tessan, has even shown that the method of least squares is here applicable;* but M. Beaupré adhered generally to the graphic method, which he employed with as much sagacity as precision.

The application of this rigorous method fixes the position of the principal points of the chart about to be constructed, as the tops of mountains, capes, &c. The details, such as the outline of coasts, course of rivers, &c., are afterwards described with such degree of precision as time permits; and when a sojourn of some duration renders it practicable to add the soundings taken at sea, as was the case in regard to the straits of d'Entrecasteaux and other parts of the coasts of Van Diemen's Land, the positions of the points of sounding are fixed by reference to the principal points determined by the bearings, in accordance with the methods which will be presently indicated when we arrive at the hydrographic surveys of the coasts of France.

The bearings taken from the 19th to the 23d May, in the archipelago of Santa Crũz, enabled M. Beaupré to give a remarkable proof of his skill in applying these processes, which were then new. Faithful to his method of constructing, day by day, the chart of those parts of coasts which he would not again see, he devoted the night of the 21st to describing the details of the south coast of the island of Santa Cruz; that of the 22d was similarly occupied with the north coast; and, the ships sailing on the 23d for the Solomon islands, he applied himself, as soon as the land was lost sight of, to the definitive reduction of his chart. This, like all the rest belonging to the voyage of d'Entrecasteaux, was constructed on a scale of three lines for one minute of the equator; and as it presented, for the discussion of which we have been speaking, nearly all the cases to be met with in practice, M. Beaupré has caused it to be engraved in the 19th plate of the atlas, with all his lines of construction, as an example of his manner of operating, and it is here that he has explained his method with details at which we have only been able to give a cursory glance. They may be seen in the appendix relative to this subject at the end of the first volume of the voyage of d'Entrecasteaux, an appendix which has become the *vade-mecum*, and, if I may so speak, the *catechism* of the constructors of marine charts.

In reducing to rule, and in practicing his method, M. Beaupré fulfilled the most cherished wish of the scientific hydrographers, who, at the close of the eighteenth century, employed themselves with the means of giving to nautical science all the precision of which it is susceptible. Borda, after having placed in the hands of navigators the repeating circle of which they still make use, had recommended its employment in preference to the compass, which till then was exclusively relied on for surveys executed at sea. Flurieu had equally recommended astronomic surveys. For naturalizing these scientific processes in the practice of hydrography, it was requisite that some engineer of a peculiar aptitude should devote himself with energy and perseverance to the application of the new instruments and rigorous geometric methods adapted to the accurate measurement of angles. M. Beaupré proved fully equal to this honorable mission, and, thanks to his unceasing efforts, the voyage of d'Entrecasteaux inaugurated the opening of a new era—that of *precise hydrography.*

Like all other branches of human knowledge, hydrography has been advanced by degrees. After the invention of the compass, so far surpassed at a later stage by new instruments, the discoveries of Christopher Columbus and of Vasco de Gama gave ideas a wholly new direction. Subsequently the adven-

* See *Voyage autour du Monde, par le frégate Venus*, commandée par M. Abel Dupetit Thouars: *Physique*, par M. de Tessan, t. v., p. 238.

turous circumnavigations of the Magellans, Mendañas, Drakes, Tasmans, and Dampiers, made known the principal outlines of the two oceans, but with very imperfect exactness, as may be perceived from a glance at the old globes which are still of frequent occurrence in Paris. *That*, according to the happy expression of M. Villemain, was *the heroic age of the navigation of discovery;* the modern Argonauts went forth in their search for the golden fleece with an ardor little favorable to systematic exploration, and which yet did not prevent them from overlooking the rich auriferous deposits of California and Australia.

Towards the middle of the eighteenth century, after Buffon had published his *Natural History*, the taste for voyages was revived under a form even then much more scientific. In the course of a few years we see Byron, Carteret, Wallis, traverse the Pacific ocean, and make the tour of the world. Cook is sent to Tahiti to observe, June 3, 1769, the passage of Venus over the disc of the sun. He makes two other important voyages, and after having traversed the Pacific in all directions, and penetrated into the frozen regions of both poles, falls in 1779 beneath the weapons of the natives of the Sandwich islands. Cook remains the principal figure and characteristic of this period; but had fate permitted the instructions given to la Perouse to have been completely carried out, the voyage of this last would, perhaps, have afforded the best example of what it was possible to accomplish with the hydrographic methods then in use. These different enterprises made known almost all the lands and archipelagos with which the ocean is strown, and furnished charts which already presented their general form with a great degree of fidelity.

Last come the *hydrographic voyages of precision.* If the expedition of d'Entrecasteaux offers the first example of them, the voyage of the *Coquille*, executed under the command and published under the direction of our distinguished colleague, Captain Duperrey, must, perhaps, be regarded as the most perfect type of this class of enterprises. To the same class belong the almost too hazardous voyages of Sir John Ross among the ices of the antarctic pole, and those not less daring of M. Dumont d'Urville.

The hydrographic study of the archipelago of Santa Cruz, which retained around M. Beautemps-Beaupré some of the most skilful officers of the frigate, did not so exclusively occupy the attention of Admiral d'Entrecasteaux and other chiefs of the expedition as to divert their attention from the main object of their mission, which was to seek for traces of la Perouse. They constantly communicated with the shores, questioned the natives, examined the objects in their possession, and observed, among other things, a piece of iron from the hoop of a cask, set as a hatchet; but no one then suspected that there was here a vestige of the expedition of la Perouse. The admiral has minutely recorded the reasons why no importance was attached to the circumstance.

Nevertheless the chart of the archipelago of Santa Cruz presents, in its SE. portion, an island on which by a rather singular chance the admiral bestowed the name of *la Recherche*, after that of his own frigate sent in *search* of la Perouse. "We took the bearing of this island, says M. Beautemps-Beaupré, for the first time from our point of station at 20 minutes after 9 o'clock, 19th May, at a great distance, At noon, the same day, we again took its bearing, and then lost sight of it." Situated at the southeast extremity of the archipelago of Mendaña, this island has been in like manner seen and lost sight of by not a few other navigators in whose track it lay, and who little imagined that la Perouse and his companions had paid with their lives for the honor of having previously discovered it.

Thus two years earlier than d'Entrecasteaux, Captain Edwards, commanding the English frigate *Pandora*, had discovered, August 13, 1791, this same island, which he had named *Pitt island*, and had sailed around its southern shore without suspecting that it concealed the remains of a world-renowned shipwreck. Thirty years later, in 1823, Captain Duperrey, among whose officers was M.

Dumont d'Urville, passed in the corvette *la Coquille*, 2d and 3d August, at about half a degree to the W.SW. of the island. Strong eastwardly winds prevented him from approaching nearer, but he took numerous bearings which served to rectify the position of the island, and then obeyed without thought the wind which bore him away from it, having himself no reason for supposing that this obscure spot presented any trace of the expedition of la Perouse.

Yet the veil was about to be withdrawn. Four years after, in December, 1827, and January, 1828, M. Dumont d'Urville was lying with the *Astrolabe* in the port of Hobarttown, situated in those parts of Van Diemen's Land which MM. d'Entrecasteaux and Beautemps-Beaupré had surveyed with so much care while they were still desert. Here reports reached him, vague indeed, and even contradictory, respecting a surprising discovery made by Captain Dillon, commanding an English vessel, engaged in commerce. This mariner, it was said, had acquired authentic information relative to the shipwreck of la Perouse, and had even brought away the handle of a sword which he claimed to have belonged to that celebrated navigator.

Notwithstanding the slight authority for these reports, M. Dumont d'Urville thought himself justified in modifying the route which his instructions traced for him. He touched, February 10, at Tikopia, where he found among the natives a lascar named Joe, a sailor and native of Calcutta, who was the same that had sold the sword-handle to Captain Dillon. This man, after a little hesitation, acknowledged that some years before he had gone to the *Vanikoro isles*, which are no other than the group of *la Recherche*, where he had seen many objects belonging to the vessels of la Perouse; that he had been then told that two very aged whites were still alive, but he himself had not seen them.

The next day, February 11, 1828, the *Astrolabe* sailed for the Vanikoro islands, situated, according to the natives, about forty leagues W.NW. from Tikopia. The vessel came to anchor, February 14, at the place of its destination, and remained till the 17th of March. M. Dumont d'Urville, being quite seriously indisposed, could not quit the corvette, which, besides, was, in more than one respect, not considered in entire safety; but, after having interrogated the natives, he despatched in succession several parties commanded by responsible officers, with whom he associated his faithful surgeon, M. Gaimard, whose recent death has been a new occasion of sorrow to the friends of science.

The chain of reefs which, at a distance of two or three miles, forms an immense girdle around Vanikoro, closely approaches the southern coast near Païou, in front of a place called Ambi. Here it is but a mile off, and it was here that, on a first visit, the native who preceded M. Jacquinot stopped his canoe in an opening between the breakers, and made a sign to the Frenchmen to look beneath the water. There, at a depth of twelve or fifteen feet, were clearly distinguishable, scattered here and there, and imbedded in corals, anchors, cannons, bullets, and divers other objects, especially numerous sheets of lead; the wood had entirely disappeared. The position of the anchors seemed to indicate that four of them had sunk with the ship, while two others had probably been let go. On a second visit M. Guilbert succeeded in withdrawing from the reefs the following objects: An anchor of about eighteen hundred pounds weight, without a stock, much rusted and covered with a crust of corals apparently from one to two inches in thickness; a cast cannon, likewise covered with corals, and so much oxydized that the metal readily yielded under the hammer; a small swivel of brass and a blunderbuss of copper in much better preservation, one bearing on its trunnions 548 as its number, and 144 as its weight; the other 286 and 94 for its number and weight respectively, with no other marks; a pig of lead and large sheet of the same metal, together with some fragments of porcelain. The remains of a kettle had been previously procured at Nama, a village of the coast.

The following is the amount of the information obtained from the natives: About forty years previous to 1828, (which would carry us back to 1788, the date of la Perouse's disappearance,) one morning, at the close of a very dark night, during which the wind blew with violence from the SE., the islanders suddenly descried on the southern coast, opposite the district of Tauema, an enormous *pirogue*, stranded upon the reefs. It was rapidly demolished by the waves, and so entirely disappeared that nothing was ever recovered from the wreck. Of the persons who manned it a few only succeeded in escaping in a boat and gained the shore. The following day, likewise in the morning, a second pirogue, similar to the first, was discovered on the rocks before Païou; where, in the lee of the island, and less racked by the wind and sea, stranded moreover on a level shelf of twelve or fifteen feet depth, it remained some time in its position without being destroyed. This, like the first, bore a white ensign. The strangers who manned it landed at Païou, where they established themselves with those saved from the other ship, and immediately set about constructing a small vessel from the fragments of the ship which had not gone down. Their task was completed in six or seven moons, and, as most of the savages averred, all the strangers left the island. A few, however, declared that two remained behind, but that these had not long survived.

M. de Fromelin, who also visited these shores in 1828, on the corvette *la Bayonnaise*, and who had doubtless heard of the discovery of the English Captain Dillon, ascertained by examination the existence of the remains of the French frigate on the reefs of Vanikoro.

It was a source of regret to M. Dumont d'Urville that he had not been able, in 1828, to visit in person the place of the shipwreck; hence, when on a last and memorable expedition he traversed anew the great ocean, he caused his ships, the *Astrolabe* and the *Zelée*, to lie to, 6th November, 1838, near the reef of the southern shore of Vanikoro. Landing in a sea too rough to admit f stopping on the reef, he discovered a space cleared of trees, which appeared to him to have been the spot where the parties from the wreck had pitched their camp. Near it he observed a large cocoa-nut tree which had been deeply cut around the trunk at two metres above the ground, besides other traces of the use of the axe at a remote date, but beyond this he noticed no new indications.

The two frigates mounted with cannon, which could be none but those of la Perouse, for no others were known to have disappeared in these seas, had doubtless encountered, but with more adverse fortune, casualties similar to those which befell the frigates of Admiral d'Entrecasteaux; of which one was near being lost on the Beaupré islands at the time of their discovery, and the other struck on a reef of zoophytes in the pass which forms an entrance to the haven of Balade, but was fortunately extricated.

It was not an impossibility that the remnant of the crews of la Perouse should be saved in the bark which they had constructed, and on which they put to sea about the close of the year 1788. In fact, the English Captain Bligh, of the ship Bounty, abandoned in the midst of the South sea by his revolted crew, in an undecked shallop only twenty-two feet in length, passed, 18th May, 1789, about fifty leagues to the south, and consequently almost within sight of the isles of Vanikoro, and succeeded, May 29, in reaching the coast of New Holland at the south entrance of Torres' straits, whence they made their way to Coupang, in the island of Timor. True it is, as appears from the romantic narrative of his adventures, that not to have perished a hundred times was due only to the most astonishing good fortune. This fortune was denied to la Perouse and his companions, though the boat in which they left Vanikoro but a few months before was no doubt larger and better appointed than that of Bligh.

In similar circumstances many others have succeeded in being saved. In reading the stirring recital of their various perils, we readily perceive that in the fate of la Perouse there is nothing enigmatical; nor can the conclusion escape

us that the expedition of d'Entrecasteaux must have been conducted with as much ability as zeal, when we see on the chart of the archipelago of Santa Cruz, by M. Beautemps-Beaupré, two of the lines of survey directed by him upon the island of *la Recherche* or *Vanikoro*, meet precisely at the spot where still lie beneath the waves the anchors and cannons of one of the frigates of the illustrious and unfortunate navigator.

The ships of d'Entrecasteaux continued in sight of the island *la Recherche* almost the whole of the 19th of May, 1793. Besides the instruments of the survey, there was no deficiency of telescopes pointed towards the land, through which, if signals after the European manner had been made, the piercing eyes of some of the mariners could not fail to have descried them. But the survivors of the wreck were doubtless long departed or dead when the expedition passed, which was not till five years after the disaster. As to finding under the waters of the sea the remains of the shipwreck, that would have been a stroke of good fortune such as seems in general not to have attached to anything connected with the expedition of la Perouse. Perhaps, however, d'Entrecasteaux might have had that melancholy satisfaction, if his officers had paid more attention to the piece of iron, mounted as a hatchet, which was seen in possession of the natives of Santa Cruz, for it had very possibly been procured from the remains of the wrecked frigates. But who will venture to say that in their circumstances he would himself have divined it.

However that may be, the hour had now come for the departure of the expedition. Sailing from Santa Cruz it pursued its prescribed course, and thus separated itself more and more from the principal object of its research; yet, thanks to the indefatigable zeal of M. Beautemps-Beaupré, it continued to render eminent service to hydrography. It traversed the archipelagos of the Solomon and Louisiade groups, the coasts of New Britain and New Guinea; but a deplorable incident awaited it on these obscure shores. Admiral d'Entrecasteaux died July 20, 1793, after a short illness which presented some of the symptoms of scurvy. The captain of the frigate *l'Esperance* had already fallen a victim to fever in the port of Balade. Very soon scurvy and dysentery had decimated the crews which left France in 1791, while the loss among the higher officers divided itself with impartial severity between Paris and Coblentz. Not that there was any suspension of the surveys, which continued to produce excellent charts, but a feeling prevailed that it was time to desist. The two frigates were turned towards the island of Java, and entered the port of Sourabaya, where the voyagers learned that the day of their arrival was not only October 27, 1793, but, at the same time, the 6th Brumaire of the year II.

The expedition was here broken up and its different members returned separately to Europe. In his passage, M. Beautemps-Beaupré stopped some time at the Cape of Good Hope. He had preserved the minutes of his charts, but the fairly executed transcripts, with other scientific documents collected by the expedition, were captured on the return by the English, by whom, however, they were restored at a later period. Yet, to avoid the possibility of their disappearance, he employed the time of his stay at the Cape in making a new copy, which his friend M. Renard, chief surgeon of the expedition, undertook to convey privately to the representative of France in the United States of America. He himself embarked on a Swedish vessel, which landed him at Gothembourg, where M. Fournier, French consul, procured him the means of re-entering his own country.

Arrived at Paris August 31, 1796, after an absence of five years, he rejoined his excellent friend M. Fleurieu, and resumed, under his direction, the preparation of the *Neptune of the Baltic sea*, being at once named hydrographic engineer of the first class, and under-keeper of the general depot of the marine. In 1798 the editing and publication of the charts of the voyage of d'Entrecasteaux were officially confided to him. This great performance, which did not appear

till 1808, was a work of prolonged execution, but the co-operation which he gave it did not engross him exclusively, and from the 20th July, 1799, to the 26th June, 1804, he was charged in chief with making the hydrographic survey of the course of the Scheldt, and with a succession of other hydrographic missions relative either to the Scheldt or to the coasts of the North sea.

Admiral Rosily, director of the depot of marine, being designated at the end of the campaign of 1802 to make an inspection of these labors, informed himself of the methods followed by M. Beautemps-Beaupré, as well in fixing the positions of shoals and of soundings as in the construction of the plan. He gave his complete approbation to these methods, which consisted essentially in the combination of the accurate measurement of angles by means of the circle of reflection, with the employment of the geometric principle of the "problem of three points," a combination whose application to submarine topography is one of the best titles of M. Beautemps-Beaupré to the respectful consideration of hydrographers.

In 1804 the *Nautical Description of the Coast of the North Sea from Calais to Ostend* was published under the auspices of the depot of marine. This work gives in detail the description of the shoals which obstruct the port of Dunkirk, and of those which are comprised between Dunkirk and the entrance of the Scheldt, as well as the nautical instructions necessary for mariners who frequent those shores. The chart which accompanies it was reproduced at the hydrographical office of London, with an English title, as having been executed by *Admiral* Beautemps-Beaupré; for the English were not slow in ascertaining, though a little vaguely, that under that name there existed a hydrographer worthy of the highest confidence. In the following years M. Beautemps-Beaupré explored the course of the Scheldt, till then but little studied, and, for the first time, demonstrated the practicability of the ascent of that river by ships-of-the-line as high as Antwerp, an indication which furnished a basis for the plans of the Emperor at that point. Charts of minute detail embody the results of these labors, before the termination of which M. Beautemps-Beaupré was advanced in his position as hydrographical engineer and officer of the marine, and was named (August 5, 1804) a member of the legion of honor. He had by this time, indeed, become pre-eminently the hydrographer of the Emperor Napoleon. The latter, when a city or department required an important and difficult construction, was accustomed to say: "*I will send Prony thither.*" When the matter in hand was the elaboration of one of those great projects which he had so justly at heart for the re-establishment of our maritime power, he sent, without saying anything, M. Beautemps-Beaupré.

After the campaign of Austerlitz and the peace of Presburg, the views of the Emperor were turned towards the coasts of Dalmatia, of which the numerous inlets and islands, with their steep banks and deep channels, present magnificent harbors, equally sheltered from the wind and the enemy, and of great importance to the Venetian marine. M. Beautemps-Beaupré received (February 6, 1806) an order to make the hydrographic survey of the military ports on the east shore of the Gulf of Venice. To this object he devoted three campaigns, in 1806, 1808, and 1809. He took plans of the whole coast from Trieste to the mouths of the Cattaro, embracing the port of Pola, and the still more magnificent one of Calamota, near Ragusa. The plans and surveys of coasts which he executed have been published on a reduced scale, but the admirably drawn originals remain one of the ornaments of the depot of marine.

After the battle of Wagram he was sent by General Maureillan, governor of Zara, to the headquarters of the French army at Vienna, as bearer of a convention of armistice relative to Dalmatia. He received, on this occasion, from the hand of the Emperor, the decoration of the iron crown. Being ordered to report himself, with his charts, to the minister of marine at Paris, he had scarcely arrived at that city when he was named member of a commission

charged with duties relating to military operations on the coast of Zealand, where the English had made a descent. Recurrence to him was the invariable rule in everything bearing on the affairs of the Scheldt, and in the intervals of his labors in Dalmatia he had been repeatedly required to return thither. His indefatigable activity was equal to all demands.

A new phase in his life now opened to him. The death of his venerable master and friend, M. de Fleurieu, had left a place vacant in the first class of the Institute in the section of geography and navigation. M. Beautemps-Beaupré consented, with much distrust, to become a candidate. To make the report on his titles to a nomination fell to the lot of M. Arago, who, observing the number and variety of his labors, said to him: "*You must have lived a hundred years!*" He had lived, however, but forty-four, and was nominated, September 24, 1810, by a large majority. One of his principal competitors was Admiral de Rosily, director of the depot of marine, his official chief and constant friend. The transient rivalry produced no change in their feelings or relations. In our peaceful contests, he who loses to-day frequently succeeds to-morrow, and the merit of one aspirant places in higher relief the merits of others. Admiral Rosily was himself an hydrographer of much experience and great knowledge. In 1787, during the voyage of la Perouse, he had executed, by order of the King, on the frigate Venus, which he commanded, the hydrographic reconnaissance of the Red sea. In 1816, zealously supported by M. Beautemps-Beaupré, he, too, became a colleague of the Academy in the section of free academicians.

In 1811 the empire had been extended as far as Hamburg and Lubeck. M. Beautemps-Beaupré, who, at the beginning of his career, had labored on the *Baltic Neptune* under M. Fleurieu, was now charged with the hydrographic exploration of the northern coasts of the empire beyond the Scheldt. From 1811 to 1813 he made a series of surveys in the departments of Holland, as well as at the mouths of the Ems, the Weser, and the Elbe, in view of the establishment of a great military port. The decision, founded on his investigations, being in favor of the Elbe, he was charged with the selection of the most favorable site on the left bank of that river, and made a complete hydrographic survey of its course.

In 1815, during the hundred days, the Emperor, at a reception in the Tuilleries, stopping abruptly before M. Beautemps-Beaupré, said to him, with an air of chagrin: "*We are still very far from the Elbe—and your charts?*" "Sire," replied M. Beautemps-Beaupré, "I considered it my duty to send them to the United States by an American vessel." "*It is well,*" rejoined the Emperor, gratified at recognizing in this trait the man who had been the confidant and faithful instrument of his great designs. At a later period the charts were remitted to the government of Hanover, and M. Beautemps-Beaupré was named a member of the Royal Society of Sciences of Göttingen.

Justly honored for so long a series of services, he might have now resigned himself to a well-earned repose, but his was not the temperament for such an indulgence, and at an age when many think of closing their career he commenced a new one. Since his return from the Cape of Good Hope in 1796, he had been unable, by reason of the war, to extend his labors beyond the waters closed to the enemy, and, with the exception of his exploration of the coasts of the North sea, after the peace of Amiens in 1802, he had been obliged to confine himself to some of the rivers of Germany and the equally protected inlets of Dalmatia. The return of peace again made the ocean free, and the opportunity of revisiting it was seized with alacrity by M. Beautemps-Beaupré, for whom it seemed to revive the brightest days of his early manhood.

Admiral Rosily, director of the depot of marine, had the merit of immediately comprehending what the occasion required and allowed, and Louis XVIII that of entertaining his proposals with favor, notwithstanding the em-

barrassments of the times. The ordinance directing the immediate preparation of the *pilot of the coasts of France* was signed June 6, 1814, but the labor could not be commenced till 1816. By an ordinance of the former date, M. Beautemps-Beaupré was named hydrographic engineer-in-chief* and joint keeper of the general depot of the charts, plans, and journals of the marine.

The condition of French hydrography at that time was an anomaly resulting from circumstances. The administration of Louis XIV had occupied itself with the hydrography of the coasts of France, and the engineer Lavoye had executed, about 1670, charts of the coasts of Brittany which were quite passable, or at least very much superior to those which represented the parts of the coast comprised between the mouth of the Loire and the shores of Spain. A century afterwards, in 1776, the government ordered a hydrographical reconnaissance of the coasts of France under the superintendence of la Brettonniere, captain in the navy, and Mechain, astronomer for the marine and member of the Academy of Sciences; but it would seem that those distinguished personages were rather charged with the collection of materials for rectifying the errors of the old charts, than with the execution of such a detailed and complete survey as might meet the wants of the service under all circumstances. There remain in the archives of the depot of marine but few documents relating to their operations, which extended, however, from Dunkirk to the Bay of Cancale.

Since that time geography had made in France important advances with which hydrography had by no means kept pace. Before the close of the eighteenth century there were geographic charts of a great part of the globe, competent to convey a general and sufficiently precise idea of the continents and seas. France particularly had been enriched with the map of Cassini, known also by the name of the map of the Academy, a work of great merit for that time in point of execution, and of great utility. It may be said, however, with truth, that towards the end of the last and in the first years of the present century, the art of constructing geographical charts received improvements by which it was essentially revolutionized. This amelioration was consequent upon the establishment of the metric system, which had necessitated the measurement of the meridian of France, from Dunkirk to Barcelona, and afterwards to Formentera. To the chain of triangles established in the execution of this measurement a comprehensive triangulation was subsequently attached, extending over the whole of France, and in the sequel over considerable portions of Spain, of Italy, and of Great Britain. In the prosecution of these vast and difficult labors several members of the Academy have borne a conspicuous part: MM. Delambre, Mechain, Biot, Arago, Mathieu, Puissant, in conjunction with most of the members of the corps of topographical engineers and sundry officers of the military staff. On the triangles of the meridian has been based the trigonometric system of the new map of France, published by the depot of war. In England, savants of the highest merit, Colonels Mudge, Roy, Sabine, and the most distinguished officers of the ordnance corps, have

* It may occasion surprise that M. Beautemps-Beaupré, employed and appreciated as he was by the Emperor Napoleon I, should have retained till 1814 the title of *ingenieur-hydrographe ordinaire;* but this will be more easily understood from the following letter written July 20, 1819, by M. *le duc Decrès*, who had been minister of the marine under the empire: "All the world appreciates the services rendered by M. Beautemps-Beaupré with a zeal, perseverance, and talent above all praise; but I, who have maintained close relations with him for many years, cannot but regard him with sincere attachment, and owe him many thanks for the proofs of friendship which he has always given me. There are persons who, without the least claim, are always soliciting; these are numerous. There are others, forming but a small minority, who, with the most incontestable claims, never solicit anything. The fact is, that during the eighteen years of my official relations with M. Beautemps-Beaupré, he ceased not to occupy my attention by his labors, but never once invoked it by a solicitation. Since he forgets himself, it is but right that justice and friendship should remember him."

combined their operations with those of our own countrymen, and have commenced the publication of a magnificent chart of England, designated by the name of the *Ordnance Map*.

To place French hydrography on a level with geography, while rescuing it from the momentary abandonment which war had necessitated, was now the object of interest. The instructions which M. Beautemps-Beaupré received for this purpose were framed by Admiral Rosily, chief of the marine depot, and M. de Rossel, who had become one of its joint directors, after having aided in the hydrographical labors of the expedition of d'Entrecasteaux. These instructions indicated the west coast of France as first claiming attention, since among all those to whose hydrography navigators had need of daily recurrence, this was most noted for its defect of exploration. It was to Brest, therefore, that M. Beautemps-Beaupré repaired, and here two schooners had been built for him, whose names, *la Recherche* and *l'Astrolabe*, gratefully recalled the memory of la Perouse and d'Entrecasteaux. To these were joined the light vessels necessary for the accomplishment of his mission.

In indicating the objects proposed for his attainment, he was left at liberty to adopt that mode of operating which long experience in labors of this nature might induce him to select. He thus found himself authorized either to unite all the means placed at his disposal on a small extent of coast, in order to produce promptly a description of it, or to distribute them over several points at the same time.

The first was the mode on which he determined; as well because he had already proved, as he himself tells us,* its efficiency under various circumstances, as because it was the only one which would enable the depot of marine to publish in succession the collective results of each campaign. By concentrating the operations of the engineers successively on small extents of coast, it was in his power to verify in some measure daily the labors of each of his assistants. Thus, for instance, when an engineer, in sounding, encountered some obstruction which had escaped former researches, he gave notice of it, and M. Beautemps-Beaupré was in a position to make a personal investigation immediately. To this mode of operating he owed the advantage of being able to combine all his means at the same moment on a dangerous position, when the weather was favorable. In this way he has often succeeded in terminating in a single day, or even a few hours, the examination of dangers situated far in the offing, the description of which would have required the employment of an isolated engineer during a whole season; of this kind were the reconnaissances of the western extremity of the bank and race of Sein, the flats of Roche-Bonne, &c.

The years 1816, 1817, 1818, were exclusively devoted to the survey of the maritime position of Brest, and its results, forming the first part of the *Pilote Français*, were published in 1822. The operations of 1819, 1820, 1821, and of the first part of 1822, embraced the survey of that part of the western coast of France comprised between the point of Penmarch (Finisterre) and the isle of Yeu, (Vendée,) and furnished the materials of the second part of the above work, published in 1829. From 1822 to 1826 the survey was extended to that part of the coast comprised between the isle of Yeu and Spain, and its results appeared as the third part in 1832. In 1839 the fourth part was given to the world, representing the labors of five years from 1829 to 1833, and embracing a description of the coast between the isle of Brehat and Barfleur. In 1834, 1835, and 1836, the operations were extended from the latter point to Dunkirk. Finally, in 1837 and 1838, the survey was made of that portion of coast comprised between the isle of Brehat and the

* *Exposé des Travaux Relatifs a la Reconnaissance Hydrographique des Côtes Occidentalis de France*, par M. Beautemps-Beaupré, p. 3.

northern rocks of the Passage du Four, (Finisterre,) where operations had stopped in 1818, and thus were completed the materials for the fifth and sixth parts of the *Pilote Français*, which appeared in 1842 and 1844. The six atlases contain twenty-one general charts, sixty-five special charts, thirty-one plans of double elephant size, fifteen of half elephant, and fourteen of quarter elephant size, two hundred and seventy-nine tables of surveys taken of the principal dangers of the west and north coasts of France, and one hundred and eighty-four tables of high and low water observed during the progress of the twenty seasons spent upon the same coasts. The account (*l'Exposé*, &c.) of these hydrographical labors, executed under his orders, was so drawn up by M. Beautemps-Beaupré as to serve as the complement to the second chapter of the *appendix* attached to the first volume of the voyage of d'Entrecasteaux. In justifying this form of composition, he pleads that, when that appendix was published, his practical knowledge of the best means for reconnoitring maritime obstructions could not be so positive as that acquired during his first ten campaigns (1817 to 1827) on the coasts of France. It is certain, nevertheless, that in everything essential his method was definitely fixed at the time of the publication of d'Entrecasteaux's voyage in 1808; and in the preface to that work it is thus spoken of by M. de Rossel, an authority of undoubted competency: "Navigators will in general find in this appendix hydrographic instructions of a far more complete nature than any heretofore published. M. Beautemps-Beaupré has here given also several expeditious methods for sounding a coast and marking the depths on the chart. These methods, of which he availed himself for his operations conducted on the coast of France, (before 1808,) by order of the minister of marine, are so useful that it would be unjust to withhold them from navigators, as well as those of which he made use during the campaign."

From these judicious observations of one of the masters of hydrographic science, it will readily be inferred that the operations which M. Beautemps-Beaupré conducted on the coasts of France differ in several essential particulars from those with which he was habitually occupied in the voyage of d'Entrecasteaux. In the latter, which pertain generally to what is called *surveying under sail*, the end principally in view was to fix the position of the remarkable objects seen on the land, capes, mountains, &c., by means of observations directed towards those objects from certain points in the course of the ship, determined with especial care. The operations on the coasts of France, within an extent generally less wide and with much less rapidity, had in view to fix various points of the sea, rocks, places of sounding, &c., with reference to certain objects determined on land, mountains, steeples, semaphores, and other signals. This was almost an inverse operation to the preceding; yet this also required numerous admeasurements of angles, which were obtained with the same reflecting circle, and the geometric constructions were derived essentially from the same trigonometric principles, although the proposition of the "problem of three points" was here more frequently employed.

As the bearings taken from the sea were directed upon all the remarkable objects of the land, it was necessary that the position of these should be determined by geodetic measurements made ashore with all the precision attainable by science. For this reason a triangulation was executed on land embracing all the points of the coast. This was effected for the western coasts of France, from Brest to Saint Jean de Luz, by M. Daussy, and for the northern and southern coasts by M. Bégat, both members of the corps of hydrographical engineers of the marine. These triangulations have been connected with the grand triangulation which serves as a base for the new chart of France published by the corps of the *etat-major*, and have been found so exact that they have been finally incorporated in that fundamental system. MM. Daussy and Bégat have deduced from their trigonometric labors a complete table of the

positions of all the remarkable objects of the coasts of France which can be seen from the sea; and it was by bearings directed upon these points, rigorously determined, that M. Beautemps-Beaupré and his assistants fixed the positions of the points of the sea which were to be marked on the charts and plans. The bearings were invariably taken with the reflecting circle, in the management of which valuable and delicate instrument M. Beautemps-Beaupré had acquired great dexterity. Nor was he less expert in constructing graphically on the first rough draught of his chart the points observed by his method, founded on the geometrical principle of the "problem of three points." He was master in a surprising degree of the varied constructions deducible from this principle, and applied them, as the case might require, with the utmost readiness and sagacity.

It is usually by means of the circumferences of circles described with the observed distances that the points of station are obtained; but when this construction presents some difficulty by reason of the length of the radius of the circle, the nearness of centres, &c., it is practicable to substitute one of those somewhat numerous and generally quite simple constructions which elementary geometry deduces from the same fundamental theorem. Thus, in many circumstances, calculation may be used to find the radii and centres of the circles to be described. M. Beautemps-Beaupré recommends for these constructions, combined with calculation, the employment of the tables of natural tangents and sines.

The scale adopted by the hydrographic engineers for the first reduction of the labors was six lines for 100 toises, or $\frac{1}{14400}$, equal to six times that of the ch of Cassini. The charts, and even plans, however, have been generally published on a scale much smaller, but M. Beautemps-Beaupré soon recognized the propriety of not only collecting the materials requisite for the execution of the new charts of the coasts of France, but of exerting himself, moreover, to bring together in the archives of the depot of marine all the documents which might be useful in the sequel for forming a judgment of any projects relating to navigation. He suffered himself to be deterred neither by the difficulties nor magnitude of the work, and the depot found itself eventually in possession of a collection of five hundred and twenty-seven quarto volumes, containing the documents requisite to execute at need, on the largest scale, the plan of all parts of the western and northern coasts of France to which the attention of government might be called.

One of the most essential and useful parts of marine charts is the indication of the depths of the sea at different points obtained by the sounding line and denoted by figures on the chart. M. Beautemps-Beaupré was equally skilled in making and in marking the positions of soundings, and it is with the authority of a practised master that he recapitulates in the *Exposé des Travaux, &c.*, the rules of the difficult art of submarine topography. It was seldom that an obstruction or peril escaped him, though he seems to take pleasure in citing, for the instruction of his successors, instances in which his researches were baffled for years in succession. One day notice was given him that a vessel had touched upon a rock at a point where none was known to exist. He sought for it a long time without success, but at last his line fell upon it. The rock was simply a peak whose diameter scarcely exceeded that of the lead of the sounding line.

It is necessary to take account in soundings of the constant variations of the level of the sea by reason of the tides. "The first thing to be done," says our hydrographer, "at the commencement of a campaign, on a coast where the water through this cause continually changes its level, is to place a certain number of scales, divided into feet and inches, on which those changes shall be observed, since it is by means of observations of this kind that we are enabled, in giving the chart its definite form, to reduce to the lowest water level the soundings made at all hours of the day and tide. To reduce the soundings is to subtract from the depths found on different days and at all hours of the tide, for every

point of the coast sounded, the suitable number of feet, in order to transfer to the plan only the depths of water found at each point at the precise instant of lowest depression. The tables of high and low tide, at many principal points on the coasts of France, are extracts from the large body of observations which served for the reduction of the soundings." (*Exposé des Travaux*, &c., p. 10.)

As M. Beautemps-Beaupré has more than once remarked, the soundings in many parts of the sea are far from being necessarily unchangeable. It is readily conceived that they must vary as well from the effect of deposits produced at some points as of erosions which take place at others. He had said, as early as 1804, in his nautical description of the coast of the North sea: "We forewarn navigators that our work must not be regarded as everywhere authoritative, except for a limited time, on account of the changes which are in progress in the shoals upon these shores." To the same effect he observes with reference to the western and southern coasts of France: "All banks of sand and ooze undergo changes of position and of depth of which navigators should ever be distrustful, since the best charts can only give, as regards dangers of this kind, insufficient information when some time has intervened since their construction. And this applies especially to such banks when they obstruct the mouths of rivers. Hence the necessity of sounding annually the principal channels, and, indeed, of frequently renewing the charts of the entrances of rivers." It may be added that the comparison of the successive charts of the same region will some day furnish valuable data respecting the accumulation of sub-marine alluviums.

It was to the class of researches just spoken of that our colleague dedicated his last hydrographic labor. He had not taken final leave of the sea in closing, in 1839, his survey of the northern coast of France. In 1841, at the age of seventy-five, he cheerfully complied with the invitation of Admiral Baudin to join him in an investigation of the changes produced in the system of bars at the mouth of the Seine within the seven preceding years. It was then that for the first time he had at his disposal a vessel moved by steam, and the superior facilities thus furnished for hydrographic enterprises drew from him the remark, "That he would gladly recommence his career if it were only for the pleasure of prosecuting hydrography with such advantages."

Though he cheerfully acknowledged that the marine had done in his behalf all that was practicable, yet he had never, during his operations on the coasts of France, possessed other resources for transportation than those supplied by the sail and oar. He had generally at his command a company of eight or ten hydrographical engineers and officers of marine, and from this school of practical hydrography have proceeded many of each class who have since been intrusted with the most important labors in remote as well as neighboring seas. Among them have appeared at different times our present colleagues, M. Daussy, Admiral du Petit Thouars, and M. Dortet de Tessan; MM. Givry and Gressier, to whom was intrusted, under our distinguished and regretted colleague, Admiral Roussin, the hydrography of the coasts of Brazil; MM. Monnier and Le Bourguignon Duperré, who have furnished us a magnificent chart of our colony of Martinique, and have commenced the hydrographic survey of our Mediterranean coasts, and those of Italy; MM. Begat, Keller, Chazallon, Lisusson, Delamarche, de la Roche-Poncié, now actively prosecuting the grand hydrographic enterprises of the depot of marine; MM. Darondeau and Vincendon Dumoulin, who have so honorably associated their names with our great voyages of circumnavigation and other important labors; MM. Le Saulnier de Vauhello, Lapierre, Jehenne, De Villeneuve, who, as officers of our marine, have in different quarters of the world rendered signal services to hydrography.

Familiar with all the hazards of the sea, M. Beautemps-Beaupré exercised a consummate prudence in the employment of his assistants, and was justified in saying to the Academy, when he presented it with the sixth and last volume of

the *Pilote de France:* "It completes the satisfaction I feel at having brought to a successful close so considerable a work as that which I now submit to the Academy, that never in the course of twenty campaigns, amidst the innumerable dangers which beset our coasts, have I had to deplore the loss of one of my comrades by any accident of the sea." Nor was he less emphatic in acknowledging the zeal and science of those who had taken part in his labors, and we feel that it was with equal pride and pleasure that he took another occasion to say: "Practical knowledge may advance, and methods be hereafter improved, but we believe ourselves fully justified in affirming, that under no circumstances can greater zeal be exerted than has been displayed by all our fellow-laborers." Hence, when Louis Philippe, in 1844, named him grand officer of the legion of honor, the entire corps of hydrographical engineers felt themselves recompensed in the person of their venerable chief.

Kindness of disposition did not preclude, in the case of our colleague, great firmness of character, as was abundantly manifested amidst the vexations inseparable from labors like his; especially was his constancy of purpose proved by a circumstance which would have discouraged most others. Although he embarked young, and at the outset was tossed for two successive years on the most stormy seas, he ceased not at any time to be subject to sea-sickness, and it was amidst sufferings from this malady, which so completely subdues the stoutest spirits, that for fifty years it devolved on him to measure angles with the nicest precision, and note the details of soundings, while exposed on slight vessels to the waves which often swept over himself and his drawings; yet he paid no attention to these things, and disliked to have his infirmity observed by others. To his assistants, however, his sufferings could not be unknown, and must have contributed to the sympathetic affection with which he was regarded by those, whether officers or mariners, with whom his labors brought him into contact.

It was indeed natural that, with such a character as his, M. Beautemps-Beaupré should be loved by all who approached him, and it may be readily imagined that the 25th September, 1848, which witnessed his official retirement, was, for the depot of marine and the whole corps of hydrographic engineers, a day of undissembled regret. Equally may we conclude that it was a day of festivity when, February 2, 1853, M. Ducos, minister of marine, came, in the name of the Emperor and in the presence of the corps of engineers, to inaugurate the bust of our colleague in the grand gallery of the depot, whose invaluable documents have in great part been collected by himself or under his orders, or at least by the methods with which he has endowed hydrography. On this interesting occasion, Admiral Mathieu, the worthy and learned director of the depot, pronounced a discourse, from which I must content myself with transcribing the following passage: "In having constantly before our eyes the venerable features of him who was once our chief, and who has created that admirable hydrographic science which is the torch of navigation, we shall recall without ceasing his vast and conscientious labors, his useful counsels, his devotion to duty, his rigid probity, and at every moment of the day, so to speak, we shall pay him the tribute of respect and gratitude due to him by so many titles." To this address M. Ducos cordially responded: "This bust," he said, in closing his remarks, "is entitled to our respect, for it is that of M. Beautemps-Beaupré, so much endeared to the navigators of all nations and of every sea. In dedicating this effigy of the man of science, whom you justly consider the founder of the depot of marine, in the place which has been the witness of the labors of his long career, it would seem to be no strained metaphor which should liken this tribute of your regard and veneration to one of those beacons erected by his counsels and exertions by which you would ingeniously recall to his successors the modest point of departure, and the glorious point of success which they too may realize." The bust is perfect in its resemblance, faithfully repro-

ducing the noble features, the kindliness, united with penetration. which characterized the original. Under a physiognomy impressed with so much goodness, we are easily persuaded that we see one of those ancient savants of the primitive type whose renown is the property of ages. To the skilful statuary (M. Desprez) who executed it, the more honor should accrue, inasmuch as M. Beautemps-Beaupré had reached the age of eighty-six without having ever permitted any one to take his portrait. After the ceremony, the minister, the admiral, and the whole body of assistants proceeded to the modest residence of M. Beautemps-Beaupré, in the street *des Saints-Peres*, there to render to the illustrious old man in person, and amidst the applause of all present, an homage which must have sensibly touched his heart.

Nor were the scientific bodies, to which he belonged by more than one title, less conscientious in their acknowledgments. In 1824 he had been named member of the bureau of longitudes, and assiduously attended the meetings whenever he was in Paris. His advice in all that regarded navigation was here listened to with invariable deference. He had been also named one of the commission of light-houses from the commencement in 1826, and was especially intrusted with the suitable location of those invaluable aids to navigation. The active and influential part which he took in the deliberations of the board was warmly acknowledged at his funeral by M. Leonce Reynaud, the skilful constructor of the light-house of Brehat, the site of which was fixed by M. Beautemps-Beaupré himself, after the difficult and dangerous exploration of the *Roches-Douvres* at the entrance of the British channel. His character, his long experience of the sea, his solicitude for the public good, conspired, with the intrinsic wisdom of his counsels, to secure their constant adoption. Even on his death-bed his thoughts were still occupied with the interests and dangers of maritime enterprise; and if he manifested a sensibility, it was to the assurance that the member of the commission of light-houses had completed the work of the hydrographer, and that thenceforward all important questions bearing on the lighting of our sea-coast were resolved.

Whatever related to the sea interested him to the last. In 1853 a commission was appointed to investigate, under the direction of M. Dumas, certain questions touching the existence of the *tangue,* a product of marine origin which the sea throws up at the entrances of certain rivers of Normandy and Brittany. Agriculture dreaded the disappearance of this fertilizer. The commission, desirous of consulting M. Beautemps-Beaupré on this production of shores which he had so thoroughly explored, repaired in a body to his residence. The aged navigator recovered all his animation in speaking of places which he had so often visited: "We know not," he said, "how the tangue is reproduced at those points; it is the *fowl which lays golden eggs;* it must not be interfered with."

In the presence of the great spectacles of nature, M. Beautemps-Beaupré had contracted a taste for natural history. If he did not cultivate it himself, he zealously aided those who did. In the expedition of d'Entrecasteaux he had formed intimate relations with its botanist, M. de la Billardiere, and it was he who brought to France the beautiful *nautilus vitré* now in the Museum of Natural History which was bequeathed to the government by M. de Kermadec, captain of the *Esperance,* on his death in New Caledonia. Many of our colleagues recall with sensibility the cordial and obliging reception extended to them on our coasts by M. Beautemps-Beaupré while prosecuting his own arduous hydrographic labors.

Reared among the savants of the close of the eighteenth century, he had preserved that almost religious respect for science which was one of their distinctive characteristics. Hence the dignity, united with friendliness, which pervaded all his relations. "He was," said the Marchioness de Laplace, whose remembrance is itself a eulogy, "a man of an antique character." He possessed

that elevation of sentiment which Plutarch so well knew how to paint. Reverses of fortune, which would have overwhelmed another, were encountered by him with stoic firmness. Involved at an advanced age in the failure of a banker,* he lost by that event the savings of his whole life; but he contented himself with saying affectionately to Madame Beautemps-Beaupré, "This event, my love, makes us younger by thirty years," an expression which supposed in her an elevation of sentiment equal to his own. Few marriages, indeed, have been so happy as that which he contracted, in 1804, with Madame Fayolle, widow of a commissary general of marine. Both were nearly eighty when death separated them by the removal of the wife; it was the first cloud which had darkened their union.

M. Fayolle, issue of the first marriage of Madame Beautemps-Beaupré, found in our colleague a second father, and, as hydrographical engineer, was for many years one of his most distinguished and useful assistants.

M. Beautemps-Beaupré had always had a weakness of the breast; at the age of eighteen some physicians had even augured an early decline. When he embarked to take part in the expedition of d'Entrecasteaux, it was generally thought that he would never again see France. This prognostic was fortunately falsified; but an obstinate cough attended his whole life, and in later years subjected him to much annoyance.

It will scarcely be forgotten among ourselves that, at our sessions, he was a model of punctuality. He signed our record the 23d of October, 1853, but thenceforward was forced to renounce his attendance. This privation, and the sufferings which occasioned it, he bore with a resignation full of cheerfulness. One of our colleagues having called to see him, and expressing the hope that a strong constitution would again restore him to us, he replied with a smile, "I am duly sensible of your kindness, but I shall soon be eighty-eight." Firm in a Christian faith, M. Beautemps-Beaupré accepted death without a murmur. "Let us not repine," said Admiral Baudin at his grave, "that, in subjecting him for several months to the supreme trial of excessive suffering, God afforded him the opportunity of setting an example of resignation and unalterable serenity."

He expired March 16, 1854, surrounded by a devoted family, which numbers two inheritors of his distinguished name—M. Pierre Beautemps-Beaupré, president of the Chamber of Commerce of Grandville, and M. Charles Beautemps-Beaupré, imperial procurator at Mantes. In this Academy he succeeded M. de Fleurieu, his master and friend, and has himself been succeeded by M. Daussy, who, from 1811, had been his most constant collaborator, and who efficiently contributed to secure to the hydrographic survey of the coasts of France geodetic bases of irreproachable precision.

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* The banker, who was his relative, might have been prosecuted for fraudulent bankruptcy. M. Beautemps-Beaupré threw in the fire the only paper which could have procured his condemnation, saying, "It is not I who will ever be instrumental in disgracing a relative."

OUTLINE OF THE ORIGIN AND HISTORY

OF THE

ROYAL SOCIETY OF LONDON.

PREPARED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

"The principal advantage of academies consists in the philosophical spirit naturally engendered by them, which spreads itself throughout society, and extends to all objects. The isolated inquirer may resign himself without fear to the spirit of system; he only hears afar off the contradiction which he incurs. But in a learned society the conflict of systematic opinions soon results in their overthrow; and the desire of being mutually satisfied necessarily establishes between the members an agreement to admit nothing but the results of observation and calculation. Hence, as experience has shown, true philosophy has been generally diffused since the rise of academies. By setting the example of subjecting everything to the examination of a rigorous analysis, they have dissipated the prejudices which had too long tyrannized in the sciences, and in which the best intellects of preceding ages had shared. Their useful influence over opinion has, in our day, dispelled errors which had been received with an enthusiasm that in other times would have perpetuated them. Equally exempt from the credulity which would admit everything, and the prejudice which disposes to the rejection of whatever departs from received ideas, these enlightened bodies have always, in difficult questions, and with reference to extraordinary phenomena, wisely awaited the answers of observation and experiment, which they have at the same time solicited by prizes and by their own labors. Proportioning their appreciation, as well to the magnitude and difficulty of a discovery as to its immediate utility, and convinced by many examples that the most sterile in appearance may some day lead to important consequences, they have encouraged the research for truth in regard to all objects, with the exclusion of those only which the limits of man's understanding render forever inaccessible. Finally, it is from their bosom that those great theories have arisen whose generality places them beyond the common reach, and which, spreading themselves by numerous applications over nature and the arts, have become inexhaustible sources of light and fruition. Wise governments, convinced of the utility of such societies, and considering them as one of the principal foundations of the glory and prosperity of empires, have not only instituted them, but attached them to their own service, that they might derive from them that knowledge which has often proved of the highest public advantage."—(Laplace, *Precis de l'Histoire de l'Astronomie*, p. 99.)

"The development and advancement of science," it has been remarked, "are signally indebted to three among modern associations: the Accademia del Cimento at Florence, which endured, however, but for a short time; the Royal Society of London; and the Academy of Sciences at Paris." The first of these was established in 1657, under the patronage of the Grand Duke Ferdinand II, acting upon the advice of Viviani, the great geometrician. The name adopted by this society implies as its object the investigation of truth by experiment alone, and its members, whose number was unlimited and included the distinguished names of Castellio and Torricelli, were held to no other obligation but an abjuration of all authority and a resolution to inquire after truth, without regard to the doctrines of any previous system of philosophy. Nor did the Academy pass away without leaving a record of its labors. A volume, containing reports of the experiments made under its auspices, was printed in 1666, including, with many others, those on the supposed incompressibility of water, the universal gravity of bodies, and the property of electric substances.

For England, after Italy, is claimed a priority in the formal inauguration of a similar and purely scientific association, and the date of the establishment of

the Royal Society, which is referred to 1660, certainly preceded by six years that of its French rival. But, independently of the consideration that the period had arrived when the state of experimental science urgently demanded the realization of those splendid visions of associated activity which had long before kindled the imagination of Bacon,* the chronological origin of the illustrious bodies in question is involved in some obscurity in consequence of their previous existence as private and spontaneous reunions of certain learned men of the age. Hence the title of the "Invisible College," which we find applied by Boyle to the future Royal Society, while as yet it existed only in this inchoate state, a period of which the following passages convey to us some interesting notices:

"About the year 1645," says Dr. Wallis, "while I lived in London, (at a time when, by our civil wars, academical studies were much interrupted in both our universities,) besides the conversation of divers eminent divines as to matters theological, I had the opportunity of being acquainted with divers worthy persons inquisitive into natural philosophy and other parts of human learning, and particularly of what hath been called the *New Philosophy*, or *Experimental Philosophy*. We did, by agreement, divers of us, meet weekly in London on a certain day, to treat and discourse of such matters. Our business was (precluding matters of theology and state affairs) to discourse and consider of philosophical inquiries, and such as related thereunto, as physic, anatomy, geometry, astronomy, navigation, statics, magnetics, chymics, mechanics, and natural experiments, with the state of these studies as then cultivated at home and abroad. We then discoursed of the *circulation of the blood, the valves in the veins, the venæ lacteæ, the lymphatic vessels, the Copernican hypothesis, the nature of comets and new stars, the satellites of Jupiter, the oval shape* (as was then supposed) *of Saturn, the spots on the sun and its turning on its own axis, the inequalities and selenography of the moon, the several phases of Venus and Mercury, the improvement of telescopes and grinding of glasses for that purpose, the weight of air, the possibility or impossibility of vacuities and nature's abhorrence thereof, the Torricellian experiment in quicksilver, the descent of heavy bodies and the degrees of acceleration therein*, and divers other things of like nature, some of which were then but new discoveries, and others not so generally known and embraced as now they are."

"For such a candid and impassionate company as that was," says Dr. Sprat, in his *History of the Royal Society*, "and for such a gloomy season, what could have been a better subject to pitch upon than natural philosophy? To have been always tossing about some theological question would have been to make that their private diversion, the excess of which they themselves disliked in the public; to have been eternally musing on *civil business* and the distresses of their country was too melancholy a reflection. It was *nature* alone which could pleasantly entertain them in that estate. Their meetings were as frequent as their affairs permitted; their proceedings, rather by action than discourse, chiefly attending some particular trials in *chemistry* or *mechanics*. They had no rules nor method fixed; their intention was more to communicate to each other their discoveries, which they could make in so narrow a compass, than an united, constant, or regular inquisition. Thus they continued, without any great intermissions, till about the fatal year 1658, when the continuance of their meetings might have made them run the hazard of the fate of Archimedes; for then the place of their meeting (Gresham College) was made a quarter for soldiers."

"There arose at this time," says Dr. Whewell, alluding to the period antecedent to the epoch of Newton, "a group of philosophers who began to knock

* See the "New Atlantis," of Lord Bacon.

at the door where truth was to be found, although it was left for Newton to force it open. These were the founders of the Royal Society." "The men who formed the Royal Society," says Bishop Burnet, "were Sir Robert Moray, Lord Brouncker, a profound mathematician, and Dr. Ward, a man of great research, and so dexterous that his sincerity was much questioned. But he who labored most, at the greatest charge, and with the most success at experiments, was the Hon. Robert Boyle, a devout Christian, humble and modest almost to a fault." Among other names connected with the Society in its earlier stage, or at the period of its formal organization, and still memorable in science, literature, or the arts, may be distinguished those of Bishop Wilkins, Sir Kenelm Digby, Evelyn, Denham, Clarke, Cowley, Willis, Wren, Ashmole, &c.

"The first journal book of the Society, a plain unpretending volume, bound in basil, yet destined to receive great names and to be the record of important scientific experiments," opens with the date of November 28, 1660, and with the proceedings of a meeting which may be regarded as organic in relation to the form and permanence of the Society. Here it was determined that meetings should be regularly held every Wednesday during term time; that a contribution of ten shillings on admission, and of one shilling weekly, should be levied on each member, whether present or absent, as long as he should please to maintain his connexion with the association, and a list was formed of the names of such persons, known to those present, as were judged willing and fit to unite with them in their design. At a subsequent meeting a committee of three or more (as occasion might permit) was empowered to frame a constitution, which was submitted and adopted at a general meeting on the 12th of December following. By this, the standing officers of the Society were declared to be three: a president or director, a treasurer, and a register; the first to be chosen monthly, the two latter annually. An amanuensis and operator are styled "servants belonging to the Society," and receive salaries, the former 40, the latter 4 pounds per annum. The stated number of members was fixed at fifty-five, with permission that all persons of the degrees of baron or above might, at their choice, be admitted as supernumeraries. It was provided that no candidate should be elected the same day he was proposed, and that at least twenty-one members should be present at each election. For such election, the amanuensis, it is ordered, shall provide "several little scrolls of paper of equal length and breadth, in number double to the Society present. One-half of them shall be marked with a cross, and being rolled up shall be laid in a heap on the table; the other half shall be marked with ciphers, and being rolled up shall be laid in another heap. Every person coming in his order shall take from each heap a roll, and throw which he please privately into an urn, and the other into a box. Then the director, and two others of the Society, openly numbering the crossed rolls in the urn, shall accordingly pronounce the election." Two-thirds of those voting were necessary to a choice.

The Society having included, as we have seen, two poets, Denham and Cowley, among its members, was fairly entitled to a greeting from the muse. This it received through the ingenious pen of Cowley, in verses whose philosophical truth as well as originality of illustration may perhaps still justify quotation. After deploring the fate of philosophy, which for three or four thousand years had been kept by unwise or dishonest tutors in a state of nonage, he tells us:

> Bacon, at last, a mighty man! arose,
> Whom a wise king and Nature chose
> Lord chancellor of both their laws,
> And boldly undertook the injur'd pupil's cause.
> * * * * * * * *
> From the long errors of the way,
> In which our wandering predecessors went,
> And, like the old Hebrews, many years did stray

In deserts, but of small extent,
Bacon! like Moses, led us forth at last
The barren wilderness he pass'd—
Did on the very border stand
Of the bless'd promis'd land,
And from the mountain's top of his exalted wit,
Saw it himself, and show'd us it.

If the poet has somewhat overstated the claims of Lord Bacon as the herald of experimental philosophy, he seems to have been gifted with a clearer vision of the future achievements of the Society, which he thus apostrophises:

From you, great champions! we expect to get
Those spacious countries but discover'd yet;
Countries where yet, instead of Nature, we
Her image and her idols worship'd see.
* * * * * * * *
New scenes of heaven already we espy,
And crowds of golden worlds on high,
Which from the spacious plains of earth and sea
Could never yet discover'd be
By sailor's or Chaldean's watchful eye.
Nature's great works no distance can obscure,
No smallness her near objects can secure:
Ye'ave taught the curious sight to press
Into the privatest recess
Of her imperceptible littleness;
Ye'ave learn'd to read her smallest hand,
And well begun her deepest sense to understand.

Cowley possessed other claims than merely literary ones to scientific fellowship; he had taken a degree in medicine and written, elegantly at least, on plants and trees. He had besides, as Dr. Sprat assures us, accelerated the foundation of the Royal Society by the publication of a *proposition for the advancement of experimental philosophy,* which is still found among his works, and though the form of his proposed "college" was not adopted, it cannot be denied that he has comprehensively, if quaintly, stated the objects to which such an institution would necessarily be destined: "To weigh, examine, and prove all things of nature, and detect, explode, and strike a censure through all false moneys, with which the world has been paid and cheated so long, and (as I may say) set the mark of the college upon all true coins, that they may pass hereafter without any further trial. Secondly, it will recover the lost inventions, and, as it were, drowned lands of the ancients. Thirdly, it will improve all arts which we now have, and, lastly, discover others which we yet have not."

It cannot but afford a curious insight into the state of natural knowledge at this early stage of the labors of the Society, if we glance at the manner in which it proceeded to deal with the currency of which Cowley speaks, in order to explode what was spurious and accredit what was genuine. With this view a few entries from the journal are here given:

"Dr Clarke was entreated to lay before the Society Mr. Pellin's relation of the production of young vipers from the powder of the liver and lungs of vipers.

"Sir Gilbert Talbot promised to bring in what he knew of sympatheticall cures. Those that had any powder of sympathy were desired to bring some of it at the next meeting.

"The Duke of Buckingham promised to cause charcoal to be distill'd by his chymist, and to bring into the Society a piece of unicorne's horn.

"Sir Kenelm Digby related that the calcined powder of toads reverberated, applyed in bagges upon the stomach of a pestiferate body, cures it by several applications. [Digby delighted in the marvellous, and is said to have fed his wife on capons fattened with the flesh of vipers, in order to preserve her beauty.]

"A circle was made with powder of unicorne's horne and a spider set in the middle of it, but it immediately ran out severall times repeated. The spider once made some stay upon the powder.

"A letter was introduced treating of a petrified city and its inhabitants." &c., &c.

Other entries there are undoubtedly, and in greater number, which show that the spirit of inquiry was rapidly finding its true direction: Investigations of the mechanical properties of the air, by Boyle; experiments with the pendulum, by Sir Christopher Wren, who is said to have first suggested its oscillations as a standard of measure; observations on the "anatomy of trees," by Evelyn; instructions for the guidance of curious observers "in the remotest parts of the world." Even what now seem ludicrous tentatives with the powder of toads and vipers, or frivolous inquiries respecting the witch-hazel and still more wonderful *Lepas anatifera*,* it is more just to regard as obligatory and conscientious efforts to bring the questionable opinions of the day, however trivial, to the assay of direct experiment. The time will probably not soon come when science can claim absolute exemption from like humble labors; not, at least, "While," to borrow the words of Sir Thomas Brown, "the spirit of delusion, though expelled from his oracles and more solemn temples, still runs into corners, exercising minor trumperies, and acting his deceits in inferior seducers."

The Restoration, in diffusing a general sense of permanence and security, was highly favorable to the objects of the association, and Charles II had enough of curiosity, perhaps of wisdom, to look with a patronising eye on inquiries which threatened to interfere neither with his indolence nor pleasures. He held sundry communications with the philosophers, and even proposed subjects for investigation, before proceeding to what has been uncharitably called the only wise act of his reign—the incorporation of the Royal Society.

In the instrument by which this was effected, the King, after protesting his zeal for all learning, especially for those studies which aim by solid experiments to strike out something new in philosophy, or bring to perfection what already exists, *(novam extundere philosophiam aut expolire veterem,)* declares himself founder and patron of the Society, conferring on it the name of the Royal Society of London *pro scientia naturali promovenda*.†

Its government is deposited in the hands of a president and council, to the number, including the president, of twenty-one, all of whom were, in the first instance, nominated by the crown. For the succession, it is provided that an election shall annually take place on St. Andrew's day, in which a president shall be chosen from among the members of the existing council, and ten of this latter body shall be removed, and their places supplied by others; on which occasion not less than thirty-one members of the Society shall be present, (the president or his deputy being always one of them,) and a majority of that number shall determine the choice in each instance. Other officers of the Society are a treasurer, two secretaries, two or more curators of experiments, one clerk, besides two mace-bearers to attend on occasion upon the president. Power is given to the president and council to make from time to time such laws and ordinances as shall seem to them useful and necessary for the better government and regulation of the Society; and grants of certain pieces and parcels *(pecios et parcellos)* of land, of no great extent, are made to the learned body, to be held of the crown by the tenure of free and common soccage. A somewhat singular concession is that which authorizes the Society to demand the bodies of such executed criminals as may be desired for dissection—a circumstance

* Sir Robert Moray, first president of the Royal Society, signalized the meeting at which he was elected by presenting a paper relating to *barnacles*, in which he affirmed that he had himself seen, in the western isles of Scotland, trees to which were attached multitudes of shells, each containing a small but perfectly shaped sea-fowl, or solan-goose. He candidly confesses, however, that he did not see the products of these extraordinary limpets alive.

† "The epithet *natural*," says Dr. Paris, in his Life of Sir Humphrey Davy, "was here intended to imply a meaning of which few persons are probably aware. At the period of the establishment of the Society, the arts of witchcraft and divination were very extensively encouraged, and the word natural was therefore introduced in contradistinction to supernatural."

pointing perhaps to the large proportion of medical men which entered at that time into the association. Finally, it is provided that if abuses should occur or dissensions arise, the Archbishop of Canterbury and certain high officers of state shall be invested with powers for removing such abuses and deciding such controversies.*

The first president of the Society, after the incorporation, was Lord Brouacker; the secretaries, Dr. Wilkins and Henry Oldenburg; all appointed by the crown. "Some idea may be formed of the activity of the Society at this period by the following list of eight committees appointed on the 30th March, 1664: 1. Mechanical, consisting of sixty-nine members. 2. Astronomical and optical, fifteen members. 3. Anatomical, consisting of Boyle, Hooke, Dr. Wilkins, and all the physicians of the Society. 4. Chymical, comprising all the physicians of the Society, and seven other Fellows. 5. Georgical, consisting of thirty-two members. 6. For histories of trades, consisting of thirty-five members. 7. For collecting all the phenomena of nature hitherto observed, and all experiments made and recorded, consisting of twenty-one members. 8. For correspondence, consisting of twenty members." Oldenburg, about this time, received, as he tells Boyle, the agreeable assurance from his correspondents in Paris, that "the English philosophers were doing more for science than all the other nations of Europe, as well in curious and detached particulars as in the great works given to the public."

The labors of the Society were destined to be soon interrupted by the plague of 1665, which drove the members very generally from London. Oldenburg, however, remained at his post, and continued his correspondence on scientific matters during the whole period of the pestilence. When the meetings of the Society were resumed, the sources of the late calamity became naturally a subject of investigation, and on this occasion the animalcular origin of the epidemic was suggested. But "the vermination of the air as the cause of the plague" was supposed to have received its strongest confirmation in Italy, where Dr. Bacon, who had long practiced physic at Rome, was said to have observed that "there was a kind of insect in the air which laid eggs hardly discernible without a microscope; which eggs being, for an experiment, given to be snuffed up by a dog, the animal fell into a distemper accompanied with all the symptoms of the plague." Hooke, however, had observed that, during the summer in question, there was, in London at least, a very great scarcity of flies and insects.

A second interruption of the meetings was occasioned by the "great fire" of the following year, for, though the apartments of the Society in Gresham College escaped, that edifice was required for the purposes of the corporation of London. A removal of the meetings to Arundel House, at the invitation of its owner, led in the sequel to a donation of his valuable library, which thus became the nucleus of that of the Society. The collection consisted of 3,287 printed books, chiefly first editions after the invention of printing, besides 544 volumes of Hebrew, Greek, Latin, Turkish, and other rare manuscripts, of which the greater part, of both classes, had been purchased in Vienna by an ancestor of the noble house, and comprised the curious and costly collection formed by the celebrated Matthias Corvinus, King of Hungary. About this time also the foundations of a museum, or "collection of natural things," was formed, which, it will not surprise us to be told, comprised, among other articles, "the stones taken out of Lord Balcarres's heart, a bottle full of stag's tears, a petrified fish, the skin of an antelope which died in St. James's park, a petrified fœtus," and other equally extraordinary objects, which the language of the age not unaptly termed "rarities." The rival museum of the Tradescants already contained "two feathers of the phœnix tayle" and "a natural dragon!"

* The charters and statutes of the Society may be seen at large in the appendix to WELD'S HISTORY OF THE ROYAL SOCIETY, a learned and interesting work, on whose statements the present brief account is founded.

A subject which at this time attracted general attention was the *transfusion of blood* from the veins of one animal to those of another as a means of restoring health or prolonging life. As usual, the most extravagant expectations were indulged by the unreflecting in regard to the efficacy of this process, and the Society, rightly judging the verification of its virtues to fall within their domain, after trying with impunity the experiment of transfusion on that customary victim of scientific curiosity, the dog, set themselves in quest of a human subject for further investigation. It was first proposed to try the practice upon "some mad person in Bedlam," probably with a view to test the effects upon the mental as well as bodily sanity, but the physician in charge of the hospital refused his assent. A poor student was, however, soon found, who, for the price of a guinea, consented to undergo the operation, and indicated a sheep as the animal whose blood he was willing to receive. The experiment was conducted at Arundel House, in the presence of the Society and of other distinguished individuals, and was attended with such encouraging circumstances as to lead to its repetition some weeks afterwards, on which occasion eight ounces of human blood were taken, and about fourteen ounces of sheep's blood injected. The patient, we are told, was "well and merry" after the operation, his pulse and appetite being better than before, but respecting the permanence of these good results we are left somewhat in the dark. The condition of the patient's mind, as well before as after the experiment, may be judged of from the mystical reason he assigned, when questioned why he had elected to have the blood of a sheep transfused rather than that of some other creature: *Sanguis ovis symbolicam quandan facultatem habet cum sanguine Christi, quia Christus est Agnus Dei.* The Society had thus far met with better fortune than some of the cotemporary inquirers in both Germany and France, where death had in more than one instance been the result of similar proceedings, exposing those who conducted them to the danger of prosecution for manslaughter. Tidings of these disasters at once turned the current of public opinion in England, and led to the abandonment of further investigation on the part of the savants.

There can be no doubt that the inquisitive spirit of the Society, though often directed to subjects which no longer appear either dignified or important, had already exercised the happiest influence on the course and habits of public thought. Inquiry was propagated, and a salutary scepticism everywhere manifested its encroachments on the domain of popular delusion. Under this point of view, the historian of the Society is justified in signalizing the fact that although "during the civil wars upwards of eighty individuals were executed in Suffolk alone for supposed witchcraft, there were but two witches executed in England after the Royal Society published their *Transactions*." A body which at once prosecuted researches on the theory of eclipses, the nature of comets, and the causes of pestilence, could afford but little countenance to the widespread delusion which associated the last of these phenomena in some mysterious concomitance with the two former. When even the scrupulous Boyle had thought fit to give to one of his scientific treatises the title of *The Sceptical Chemist*, there could be not much hope for alchemy and its attendant frauds. In other fields, too, the habits of philosophical speculation which, if the Royal Society did not introduce it; at least effectually promoted by influence and example, gave rise to reforms which, as Buckle remarks,* rendered the reign of the mean and spiritless voluptuary Charles II one of the brightest epochs in the national annals, with reference to laws then passed and principles then established.

The zeal of the Society for furthering and stimulating experimental inquiry was manifested at an early period by the adoption of a resolution "that such of the Fellows as regarded the welfare of the Society should be desired to oblige

*History of Civilization in England, vol. I, p. 275.

themselves to entertain it, once a year at least, with a philosophical discourse, grounded upon experiments made or to be made; and in case of failure, to forfeit £5." This voluntary engagement on the part of Fellows, deemed "able and likely" to furnish such discourses, was at the same time made an imperative obligation on each member of the existing council. For one, the indefatigable Hooke is recorded in the journal-book as having produced new experiments and inventions at almost every meeting. An agent was salaried to traverse England and Scotland in search of zoological and botanical specimens, and this at a time when a default on the part of many members in the payment of the weekly subscription had so crippled the resources of the Society as to render even its existence precarious. An active foreign correspondence had contributed to secure to it an influence abroad scarcely inferior to that which it enjoyed at home, as was testified by the learned of Europe, among others by Leibnitz, Malpighi, and Leuwenhoeck, in the dedication of their works to the Society, or a submission of their labors to its judgment. It is a coincidence not unworthy, perhaps, of notice, that about the time when "one Mr. Leuwenhoeck," as we find him called in the correspondence, recommended to the notice of the Society his improved microscope, by the assiduous use of which he eventually arrived at the distinction of being esteemed "the father of microscopical discoveries," a "poor Cambridge student," named Isaac Newton, presented to it his reflecting telescope, "the first perfect reflector known, and made by the hands of Newton himself."* Thus science was simultaneously endowed with the perfected means of realizing both terms of Cowley's poetical prophecy—the penetration alike "of the crowds of golden worlds on high," and "the recesses of nature's imperceptible littleness." The presentation of the telescope was soon followed by the adoption of the inventor into the Society, the year 1671 being the date of the accession of the great philosopher, destined, in the eloquent language of Dr. Young, "to advance with one gigantic stride from the regions of twilight into the noonday of science."

From this period the history of the Royal Society becomes so thoroughly interwoven with the general history of science that it is manifestly impossible, in a sketch necessarily confined within the narrow limits of the present, to do more than touch upon a few prominent points illustrative either of the progress of the Society or of the knowledge which it has cultivated.

On the 8th of February, 1671–'72, Newton communicated to the Society his investigations respecting "light, refractions, and colors, importing light to be not a similar, but a heterogeneous thing, consisting of difform rays." For these discoveries the author received the "solemn thanks" of the Society, at whose request they were published in the *Philosophical Transactions*, being the first of Newton's productions which saw the light. His experiments had been made in 1666, when he was only twenty-three years of age. No sooner, however, was his theory of light given to the world than it was vehemently attacked, both as regarded his conclusions and the accuracy of the experiments from which they had been deduced; Hooke and Huyghens appearing among the number of his assailants. So true is it, as Biot has remarked, that "by unveiling himself Newton obtained glory but at the price of his repose."

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* Newton's telescope, says Weld, was the first reflecting telescope directed to the heavens, though James Gregory had previously (1663) described the manner of constructing one with two concave specula. Newton perceived so great disadvantages in Gregory's plan, that, according to his own statement, "he found it necessary to alter the design, and place the eye-glass at the side of the tube, rather than at the middle." Newton's mechanical labors led to his being sometimes regarded abroad as a maker of telescopes, and we find him styled in a book of that period, *Artifex quidam anglus nomine Newton*. It is suggestive to consider into what gigantic proportions the instrument constructed by the Cambridge student has been developed under the hands of Herschel and Rosse. Newton's first telescope is nine inches long; the length of Lord Rosse's six-feet reflector is sixty feet.

In 1686 the MS. of Newton's immortal work, *Philosophiæ Naturalis Principia Mathematica*, was presented to the Society; and being accepted with thanks, it was ordered "that the printing of the book be referred to the consideration of the council, and that it be put into the hands of Mr. Halley to make a report thereof." The council, duly sensible of the slenderness of the Society's finances at that time, were glad to devolve upon Halley, who agreed to accept it, the "business of looking after and printing the work at his own charge." In the course of the preparations for that purpose, it became necessary for Halley to inform the author that Hooke claimed to have "some pretensions upon the invention of the rule of the decrease of gravity being reciprocally as the squares of the distances from the centre," though he admitted the demonstration of the curves generated thereby to belong wholly to Newton. When apprized of this claim, the illustrious geometer determined upon the suppression of the entire third book of the Principia. "Philosophy," he said, "is such an impertinently litigious lady, that a man had as well be engaged in lawsuits as have to do with her. I found it so formerly, and now I am no sooner come near her again but she gives me warning." In the controversy relative to his optical discoveries he had written to Oldenburg: "I intend to be no further solicitous about matters of philosophy, and therefore I hope you will not take it ill if you find me never doing anything more in that kind." It required much remonstrance and entreaty on the part of Halley to induce Newton to abandon his intention of suppressing the third book, *De Systemate Mundi*, without which the celebrated work might have borne the title, *De motu Corporum Libri duo.* In view of all the circumstances it is difficult to deny the justice of the remark made in Regaud's *Essay on the First Publication of the Principia*, that "it is hardly possible to form a sufficient estimate of the immense obligation which the world owes in this respect to Halley, without whose great zeal, able management, unwearied perseverance, scientific attainments, and disinterested generosity the *Principia* might never have been published."

Halley had been elected a Fellow of the Society in 1678, on his return from his voyage to St. Helena, made chiefly with a view to astronomical observations, of which the fruit remains in his *Catalogus stellarum australium*, but rendered subservient also to the science of terrestrial magnetism, of which he is styled by a high authority the father and founder. "To him," says Sir John Herschel, "we owe the first appreciation of the real complexity of the subject of magnetism. It is wonderful, indeed, and a striking proof of the penetration and sagacity of this extraordinary man, that with his means of information he should have been able to draw such conclusions, and to take so large and comprehensive a view of the subject as he appears to have done." Halley's communications to the Society on this subject consist of a chart, the first of its kind, showing the variation of the compass, based on the idea of employing curves drawn through points of equal declination, and of papers published in the 180th and 195th numbers of the *Philosophical Transactions*. In the last of these occurs a striking passage, in which he expresses his belief "that he has put it past doubt that the globe of the earth is one great magnet, having four magnetical poles or points of attraction; near each pole of the equator two; and that in those parts of the world which lie near adjacent to any one of those magnetical poles, the needle is chiefly governed thereby, the nearest poles being always predominant over the more remote." Amid the efforts which are now directed to this subject, it will not be uninteresting to observe with how much modesty this early explorer defers the solution of his difficult problem to later times. "The nice determination," he says, "of this and of several other particulars in the magnetic system is reserved for remote posterity; all that we can hope to do is to leave behind us observations that may be confided in, and to propose hypotheses which after ages may examine, amend, or refute." And he proceeds to urge upon all navigators and lovers of natural truths to

make or collect observations of this kind in all parts of the world, and to communicate them to the Royal Society, "in order to leave as complete a history as may be *to those that are hereafter to compare all together, and to complete and perfect this abstruse theory.*"

Another science which at this era engaged the attention of the Society was geology, or, as it was then termed, "the Natural History of the Earth;" the chief representatives of which, before the Society, appear to have been Dr. Lister and Dr. Woodward. Of the former, Lyell remarks: "He was the first who was aware of the continuity over large districts of the principal groups of strata in the British series, and who proposed the construction of regular geological maps." Woodward published an essay towards a Natural History of the Earth, which attracted much attention and was elaborately reviewed in the *Transactions*. Dr. Whewell, moreover, has noted as "one of the most remarkable occurrences in the progress of descriptive geology in England, the formation of a geological museum by William Woodward as early as 1695. This collection, formed with great labor, systematically arranged, and carefully catalogued, he bequeathed to the University of Cambridge; founding and endowing at the same time a professorship of the study of geology. The Woodwardian Museum still subsists, a monument of the sagacity with which its author so early saw the importance of such a collection."*

An official connexion of the Society with the progress of astronomical observations resulted from its relations to the observatory of Greenwich (founded 1675,) of which, after having done much to sustain and advance it during the many years while it remained neglected by government, the Society finally became the formal directors or visitors by royal warrant. Under this authority the Society are required to exact from the astronomer royal for the time being an account of the annual observations made, to inspect the instruments of the observatory, and to superintend and, if deemed proper, to direct its operations. If, therefore, so eminent an authority as M. Struve has singled it out as a point well worthy of remark and encomium, that the astronomers of this illustrious observatory have maintained one unchanged system or plan in their labors during the long period from the origin of the establishment to the present day, something of this uniformity may reasonably be ascribed to its connexion with and subordination to a fixed and self-perpetuating body like the Royal Society.

The application of steam, which in our day has acquired so astonishing a development, did not fail to find among the early Fellows of the Society at least one curious inquirer, whose speculations and projects are preserved in the *Transactions*. Dr. Papin, inventor of the well-known digester for softening bones, and whose "philosophical supper" prepared upon that plan may still be enjoyed by the readers of Evelyn's *Diary*, is noticed in 1690 as having invented a method of draining mines by the force of "vapor," in which, though much was wanting to the practical perfection of the engine, the philosophical principle of the condensation as well as elastic force of steam is observed and pointed out. At a later period Dr. Papin communicated to the Society an extension of this principle to the propulsion of boats "to be rowed by oars moved with heat," and had the honor of having his project referred to Sir Isaac Newton, from whom it received a conditional approval.†

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* Woodward, whatever his scientific merit, seems to have been of an irascible temperament. He was expelled from the council of the Society for insulting Sir Hans Sloane and refusing to apologize. He fought a duel with Dr. Mead, occasioned by a dispute, as Voltaire says, *sur la maniere de purger un malade*. Woodward's foot slipped and he fell. "Take your life!" exclaimed Meade. "Anything but your physic," replied Woodward.

† The better known project of Savery, whose engine was able, through the introduction of a vacuum, to perform double the work of that devised, at a still earlier day, by the Marquis of Worcester, was exhibited before the Society (1699,) and the certificate granted by that body to the ingenious contriver, was the means of his obtaining a patent from the Crown for the manufacture of steam-engines.—Weld, I, 357.

While the Society was thus pursuing its diversified and prosperous career, Charles II, "founder and patron," had died, having entertained no intercourse with the learned corporation during his later years, except to send it a receipt for the cure of hydrophobia, compounded, after the manner of that time, of as many simples (agrimony roots, dragon roots, star of the earth, &c., &c.) as could well be disposed of in one preparation. Lord Brouncker had resigned the presidency after fifteen years of acceptable service, and had been followed in succession by Sir Joseph Williamson, (1677,) Sir Christopher Wren, (1680,) Sir John Hoskins, (1682,) Sir Cyril Wyche, (1683,) Samuel Pepys, (1684,) Lord Carbery, (1686,) Lord Pembroke, (1689,) Sir Robert Southwell, (1690,) Lord Halifax, (1695,) and Lord Somers, (1698.) The Society was soon to remove from the precarious quarters which it had heretofore occupied to a house of its own in Crane Court, Strand, and, as appears by one of its statutes, had found reason to place some further restriction on the too indiscriminate and easy admission of Fellows.

On the withdrawal of Lord Somers, in 1703, Sir Isaac Newton was elected to the presidency, the duties of which he continued to fulfil for 24 years with exemplary punctuality. His treatise on *Opticks* was now presented to the Society, a work prepared long before, but which he had decided to withhold from publication during the lifetime of Hooke. The remark suggested by the death of that able but morose and jealous man of science seems, therefore, to be fully justified: *La Société y gagne plus que la geometrie n'y perd;* but, as if the sensitiveness of Newton was doomed never to be freed from importunate molestation, the dispute respecting the authorship of the *Infinitesimal analysis* soon supervened; a dispute in which Newton, indeed, maintained his usual reserve, but which his own partisans, equally with those of Leibnitz, conducted with so much asperity and prejudice that the contest might have seemed one of honor or interest between Germany and England.* At the instance of Leibnitz, a committee was appointed by the Royal Society, in March, 1712, to examine the evidence bearing on the matter in question, and, in April following, were submitted, in a report, the reasons which led the committee "to reckon Mr. Newton the first inventor." That this did not satisfy or silence the partisans of Leibnitz will be readily believed; but at this distance of time we may acquiesce in the opinion pronounced by the historian of the Society, that Newton was the *inventor* of Fluxions as early as 1666, but that Leibnitz has the merit of having first given full publicity to his discovery of the Differential Calculus in 1673. "Had Newton done this," says Mr. Weld, "a controversy, painful in its nature and unsatisfactory in its results, would have been avoided. But all admit that he labored more for the love of truth than of fame; and this is one of the reasons why Newton is the greatest of philosophers."

This great man died on the 20th March, 1727, being, perhaps, the only one who has ever lived whose genius and virtues could sustain the exaltation of his epitaph: *Sibi grantulentur mortales tale tantumque exstitisse humani generis.* He was succeeded in the presidency of the Society by Sir Hans Sloane, who had long acted as secretary and vice-president, and whose merits as a botanist, habits of business and official assiduity, relieved the council of embarrassment in a choice, even after Newton. Martin Folkes, (elected 1741,) Earl of Macclesfield, (1752,) Earl of Morton, (1764,) James Burrow, (1768,) James West, (1768,) Sir John Pringle, (1772,) bring down the succession in the presidency to the protracted official term of Sir Joseph Banks, (1778—1820.) There are points of interest, however, in this long interval, upon which it is proper to touch even in so rapid a sketch as the present.

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* M. Arago seems to have been willing to make France a third party to this memorable competition, for, in his *Notices Biographiques*, vol. III, p. 522, he brings forward the claims of his countryman, Fermat of Toulouse, as an earlier inventor of the Calculus than either Newton or Leibnitz.

From an early date the Society seems to have labored under two especial causes of embarrassment: want of pecuniary means, arising chiefly from the failure of members to pay the stipulated contribution, and a constant tendency to extend the honor of membership to persons whose pretensions were of doubtful validity. We learn with some surprise that, while a few were occasionally exempted from the payment of the small weekly contribution, (among whom at one time was Sir Isaac Newton,) there were many others of ample means who suffered their liabilities to accumulate until the Society, to which no doubt they prided themselves in belonging, was reduced almost to the point of inaction. Nor does it increase our respect for this class of delinquents to find that when, in 1728, the Attorney General had given his opinion that the Society was authorized to sue for such arrears, and steps were taken for that purpose, the liabilities were generally discharged and the Society placed in comparative ease.* The extent of the second inconvenience may be appreciated from a saying ascribed to D'Alembert, who, in allusion to the extreme prodigality with which the honors of the Fellowship were distributed, used "jocularly to ask any person going to England if he desired to be made a member of the Society, as he could easily obtain it for him, should he think it any honor." The necessity, therefore, for some additional restriction being sensibly felt, the Society sought legal advice as to their powers in that regard, and were advised that, while their charter did not appear to authorize them to limit the Fellows to a certain number, it clearly empowered them to describe and ascertain the qualifications of persons to be elected. A statute was thereupon enacted, which has since been steadily observed, by which it is required that all candidates, except peers and some other privileged persons, shall be proposed at a meeting of the Society by three or more members, and that a paper signed by them and setting forth specifically the qualifications of the candidate, "shall be fixed up in the meeting-room at ten several ordinary meetings before the said candidate shall be put to the ballot." It appears that candidates were also expected to send in a paper on the branch of science with which they were most conversant.

Another but more occasional source of disquietude has been a jealousy sometimes manifested of undue influence or irregular procedures on the part of the presiding officer. This exhibited itself to some extent even towards Newton in the course of the preliminary steps for the removal of the Society's quarters to Crane court; but it broke out with excessive violence against Sir Joseph Banks, in 1784, upon the alleged charge of improper interference with elections, and particularly of having favored the pretensions of naturalists in preference to those of mathematicians. Groundless as this charge is shown to have been,† and factious and overbearing as was the conduct of Dr. Horseley, who, with very slender scientific pretensions, affected the leadership of the mathematical party, this schism not only disturbed the harmony of the Society, but seemed for a time to threaten its stability.

The influential part borne by the Society in the introduction of the reformed calendar into England may render an allusion to it in this place not irrelevant. By this change, which took place on the 2d September, 1752, "eleven nominal days were struck out, so that the last day of old style being the 2d, the first of new style (the next day) was called the 14th instead of the 3d. The same

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* Before the incorporation, ten shillings were required of members on the admission and a weekly payment of one shilling. By the statutes of 1663 the initiatory fee is advanced to forty shillings, the weekly payments remaining as before. In 1847 the former charge had become ten pounds, and the weekly contribution been converted into an annual one of four pounds, to be paid in advance. Liberty is given to compound for the whole by the payment, in some cases, of forty, in others of sixty pounds.

† See Lord Brougham's Lives of Philosophers, p. 363.

legislative enactment which established the Gregorian year in England in 1752, shortened the preceding year (1751) by a full quarter. Previously the ecclesiastical and legal year was held to begin with the 25th March, and the year A. D. 1751 did so accordingly; that year, however, was not suffered to run out, but was supplanted on the 1st of January by the year 1752, which it was enacted should commence on that day, as well as every subsequent year."*

The attempt to retrace here, even in the most summary manner, the philosophical labors of the Society would suggest a start'ing contrast between the narrow limits allotted to this outline and the vast field which it would be necessary to traverse. How mere a catalogue of names and of terms would be the result of any attempt to recall those achievements in every department of natural science which have distanced imagination and rendered the fictions of poetry tame and spiritless in comparison! Especially would this be the case as we approached that wonderful era of discovery, the close of the eighteenth century, which suggested to Cuvier the imposing retrospect with which he opens the *Eloge* of Haüy: "The laws of a geometry, as concise as comprehensive, extended over the entire heavens; the boundaries of the universe enlarged, and its spaces peopled with unknown stars; the courses of celestial bodies determined more rigorously than ever, both in time and space; the earth weighed as in a balance; man soaring to the clouds or traversing the seas without the aid of winds; the intricate mysteries of chemistry referred to certain clear and simple facts; the list of natural existencies increased tenfold in every species, and their relations irrevocably fixed by a survey as well of their internal as external structure; the history of the earth, even in ages the most remote, explored by means of its own monuments, and shown to be not less wonderful in fact than it might have appeared to the wildest fancy: such is the grand and unparalleled spectacle which it has been our privilege to contemplate!"† And in the realization of each and all of these surprising results the Royal Society of London has borne its effective part, yielding to none in the reflected lustre of its long line of brilliant names: its Herschels, Bradleys, Maskelynes, Youngs, Priestleys, Daltons, Watts, Wollastons, Davys, Bucklands, Murchisons, Faradays, and Airys. For, as the illustrious savant just quoted has elsewhere said with equal force and generosity, "The philosophers of England have taken as glorious a part as those of any nation whatever in the labors of the intellect which are the common heritage of the civilized world; they have dared the ices of either pole, nor is there any nook of the two oceans which they have not visited; they have multiplied tenfold the catalogue of the kingdoms of nature; by them the heavens have been made populous with planets, satellites, and stupendous phenomena; they have counted, so to say, the stars of the galaxy; if chemistry has assumed a new face, the facts which they have furnished have essentially contributed to the transformation; to them we are indebted for inflammable air, pure air, phlogisticated air; they have discovered the decomposition of water; new metals in large number have sprung from their analyses; by them only has the nature of the fixed alkalies been demonstrated; finally, at their voice, mechanics has become pregnant with miracles, and placed their country above all others in nearly every species of productive industry."‡

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* Herschel's *Astronomy*, p. 413. So great was the popular repugnance to the change of the style or calendar, that the mob pursued the minister in his carriage, clamoring for the days by which, as they supposed, their lives had been shortened; and the illness and death of the astronomer Bradley, who had assisted the government with his advice, was attributed to a judgment from heaven. It is also related that when the grandson of Lord Macclesfield, who had likewise been prominent in effecting the change of style, was standing a contested election for Oxford, the mob insultingly called out to him, "Give us back, you rascal, those eleven days which your grandfather stole from us."—*Weld.*

† See Smithsonian Report, 1860, *Memoir of Haüy.*

‡ Cuvier, *Eloge* of Sir Joseph Banks.

Among the incentives and rewards of scientific research employed by the Society are three medals, derived from funds bequeathed or granted for that purpose. 1st. The Copley medal, the fruit of a legacy bequeathed in 1709 by Sir Godfrey Copley, and termed by Sir Humphrey Davy "the ancient olive-crown of the Royal Society," being regarded as the most honorable within its gift. This has been annually awarded, with a few intervals, since 1736, in conformity with a resolution then adopted by the Society, "that the medal should be adjudged to the author of the most important scientific discovery or contribution to science, by experiment or otherwise." It cannot but be peculiarly gratifying to an American to find that when, in 1753, on the death of the surviving trustee of the legacy, the adjudication devolved on the president and council for the time being, the first award of the medal was made to Dr. Franklin. On this occasion the Earl of Macclesfield, in his address as president, stated that the council, "keeping steadily in view the advancement of science and useful knowledge, and the honor of the Society, had never thought of confining the benefaction within the narrow limits of any particular country, much less of the Society itself." The money value of this medal is five pounds, and it bears as a legend the motto of the Society, *Nullius in verba*. 2d. The Rumford medal, derived from the interest of a fund of £1,000, given by Count Rumford, in 1796, for the purpose of promoting discoveries in heat and light. This premium is duplicate, consisting of two pieces struck in the same die, the one of gold, the other of silver, and by the terms of the gift is to be awarded "once every second year." The device on this medal is a tripod with a flame upon it, and the inscription from Lucretius, *Noscere quæ vis et causa*. It is gratifying to note that the first adjudication of this prize was justly made to the founder himself, "for his various discoveries respecting light and heat," while the names of Malus, Fresnel, Melloni, and Biot, among later competitors, show that this, too, is freely accorded to foreign merit. 3d. The Royal medal, which, again, is duplicate, consisting of two gold medals of the value of fifty guineas each, a beneficence projected by George IV in 1825, though not actually realized till the reign of his successor. These medals, bearing on one side the likeness of the reigning monarch, and on the reverse the figure of Sir Isaac Newton, with emblematical accompaniments, are given for such papers *only*, on important and completed discoveries, as have been presented to the Royal Society, and inserted in their *Transactions*. Here, also, the distinguished names of Struve, Encke, Mitscherlich, and De Candolle, in the list of recipients, apprise us that this recompense has been liberally offered to the competition of all countries.

The subjects for which these prizes have been awarded are almost too multifarious for classification, and afford no indifferent criterion of the astonishing progress which has been made "since the day when the founders of the Royal Society went forth to collect May dew for its supposed cosmetic virtues, or with the *Virgula divina* in search of the hidden treasures of the earth." Yet those early inquirers are perhaps not less entitled to honor for the fidelity and heroism (for heroism it was at that epoch) with which they adhered to experiment amidst the difficulties and obscurity which surrounded them, than those who, following them in the use of the same irresistible instrument, continued to press forward with firmer and more rapid steps in the pursuit of abstract science, as if conscious that in *that* and its applications rested the sole hope for mankind of any real and sustained progression. Nor can either of the two classes cited justly claim pre-eminence over the intrepid explorers of to-day, who, undeterred by the seemingly exhaustive research to which the heavens and the earth have been subjected, still lift their minds to new and mightier enterprises, and, having encircled the entire globe with observatories and observers, shrink not from grappling with problems as subtle and inconstant as magnetism or the winds, and vast as the secular movements of suns and constellations.

"When we reflect," says Mr. Weld, "on the benefits conferred on mankind by the discoveries of modern science, Englishmen must feel an honest pride in the fact that so large a proportion have emanated from the Fellows of the Royal Society. Nor will that pride be diminished, when it is remembered that from first to last the Society has received no annual pecuniary support from government, nor assistance of any kind, beyond the grant of Chelsea College, shortly after their incorporation, and more recently, the use of the apartments they now occupy in Somerset House.* While the members of the French Institute receive a yearly stipend, the Fellows of the Royal Society pay an annual sum for the support of their institution and the advancement of science. It would be repugnant to the feelings of Englishmen to submit to the regulations of the Institute, which require official addresses, and the names of candidates for admission into their body, to be approved by government before the first are delivered or the second elected. The French *savans* are, it is true, ennobled and decorated by orders, which the wiser among them, in common with true philosophers of any country, regard with indifference. Nobly did Fourier say of Laplace: "Posterity, which has so many particulars to forget, will little care whether Laplace was for a short time minister of a great state. The eternal truths which he has discovered, the immutable laws of the stability of the world, are of importance, and not the rank which he occupied."

As a consequence of this independence and self-support, it was necessary that the Royal Society should be numerous, and by a consequence not less necessary, as Cuvier remarks, "that, as in all political associations where the participation of the citizens in the government is in inverse ratio to their number, those to whom the Society intrusts its administration should exercise over its labors, and to a certain extent over the course and progress of science, an influence more considerable than can be readily conceived of by the academies of the continent." That the Society has been fortunate in the zeal and ability of those called to preside over it, will have been observed in the course of the preceding sketch. It remains to be added that, on the death of Sir Joseph Banks, in 1820, the chair was for a short time occupied by Dr. Wollaston,† followed in the same year by Sir Humphrey Davy; by Davies Gilbert, in 1827;‡ the Duke of Sussex, in 1830; the Marquis of Northampton, in 1838; Earl of Rosse, in 1849; Lord Wrottesley, in 1854; Sir Benjamin Brodie, in 1858; and General Sabine, in 1861. The latter still worthily occupies the chair.

As something has been said above of financial embarrassments at an earlier period of the Society, it is gratifying to state, on the authority of Mr. Weld, referring to the year 1848, that this condition of things is wholly changed; besides certain tracts of land, the Society then held in the public funds upwards of £33,000; its income being derived from rents, dividends, annual subscriptions, admission fees, compositions, and sale of *Transactions* and *Proceedings*. The number of Fellows, at the same date, was 821, of whom thirteen were honorary and forty-seven foreign. The library of the Society, then containing upwards of 40,000 volumes, is extremely rich in the best editions of scientific books. Fellows are allowed to borrow books under certain regulations, though still more use is made of the library for purposes of reference.

The sessions of the Society commence in November and continue until June. At the ordinary meetings, after the usual preliminary business, one of the secretaries announces the presents made to the Society, which are so numerous that

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* Whither the Society removed in 1780.

† In reference to the extraordinary tact and acuteness of Wollaston as a physicist, it was said by Magendie that "his hearing was so fine he might have been thought to be blind, and his sight so piercing he might have been supposed to be deaf."

‡ Mr. Gilbert will be remembered by Americans as having pronounced the eulogy on Smithson, contained in the first Smithsonian Annual Report.

their titles fill, on an average, two folio pages weekly during the session. Certificates of candidates for election are then read, and next such paper or papers as may have been communicated to the meeting. For these papers formal thanks are returned, and they become thenceforth the property of the Society. Discussion on the subject treated of in the paper follows, after which the meeting is adjourned, and the Fellows repair with their friends to the library where they partake of tea, a custom introduced, it is stated, by Sir Humphrey Davy. A *conversazione* ensues, which lasts until about eleven o'clock. The council meets monthly, or more frequently, if necessary. The scientific committees assemble as occasion requires. Those annually appointed are: Mathematics, astronomy, physics, chemistry, geology, botany, zoology, and animal physiology. The number of members varies from fifteen to thirty, the latter number representing that of physics which is the largest. The *Philosophical Transactions* are generally published in two parts, (June and November,) which form a volume, though occasionally a third or even a fourth part appears. Besides the *Transactions*, abstracts of the papers and minutes are published monthly, and these, now extending to more than ten volumes, are entitled *Proceedings of the Royal Society*.

A BRIEF SKETCH

OF THE

MODERN THEORY OF CHEMICAL TYPES.

BY CHARLES M. WETHERILL, PH. D., M. D.

AFTER the electric current had been applied to the decomposition of inorganic bodies, and it had been discovered that hydrogen, the metals, and the bases appeared at the negative pole, while oxygen, chlorine, and the acids were manifested at the positive pole, the assumption that electrical attraction was the bond of union in chemical combinations was very natural, and the electrochemical theory growing out of these experiments became speedily adopted by chemists. The theory explained satisfactorily all known phenomena; it gained additional support from the discovery that the chemical elements and compounds were separated by electricity from their combinations in the ratio of their equivalents. In those days it was assumed, and at the present time it is manifest, that any theory not embracing organic as well as inorganic compounds would be untenable, and hence arose the radical theory, first applied to inorganic salts, but afterwards thoroughly studied and developed in respect to organic compounds.

As the present sketch is intended less for chemists than for others who may be confused at the appearance of the formulæ of organic compounds given in modern chemical essays, the author may be pardoned in citing facts and formulæ trite to chemists. He would also take occasion here to accredit to the Lehrbuch of Graham Otto many of the illustrations, as well as some of the arguments, employed in the present sketch.

The nature of electrical attraction renders the idea of *binary* compounds in chemistry imperative, if we assume that electricity is the bond of union in such compounds.

Berzelius imagined the elementary atoms laden with electricity and with positive and negative poles, but so that in the atom of one element the positive electricity predominated, while in that of another element the negative electricity was in excess. This excess of (+ or —) electricity communicated its characters to the element, making it positive or negative. If two atoms of different electrical character are brought sufficiently near to each other, they mutually attract each other, forming a compound atom, which is itself positive or negative according to the predominance of one kind or the other of its electricity. The new compound atom was, therefore, susceptible of further attraction by another compound atom of different electricity, and so on, the attraction becoming weaker as the compound atom becomes more complex.

Ampère imagined the atoms of positive elements to have positive nucleï with negative atmospheres or envelopes, and atoms of the negative elements to have negative nucleï and positive envelopes. Hence a positive and a negative atom upon coming together would mutually polarize each other; the + and — E of their nucleï would draw them together to form a compound, and the ± E of

their respective envelopes would be driven off and combine to produce the electrical phenomena always attending chemical action.*

According to this view all chemical compounds are *binary;* they are capable of being decomposed by the electric current, which attracts the atoms from each other according to their character, the positive appearing at the negative pole, the negative at the positive pole.

In writing formulæ the positive atom or atom group is placed BEFORE the negative one, thus: $\overset{+}{K}\overset{-}{O}$; $\overset{+}{S}O_3$; $\overset{+}{KO}, \overset{-}{SO_3}$.†

The most complex formulæ are constructed according to this binary method. In alum $= KO\ SO_3 + Al_2O_3\ 3SO_3 + 24HO$ the sulphate of potassa atom is positive, and united to an electro-negative atom of sulphate of alumina to form a still more complex atom of positive character, which is united to the negative group of atoms 24HO. When, however, the atom becomes so complicated, it is difficult to determine the electro-chemical character of its imme-

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* For views as to polarity in chemical compounds, see the excellent treatise upon the catalytic force, by T. L. Phipson, Smithsonian Report for 1862, page 395.

† For the convenience of those whose memory may require refreshing as to chemical symbols and combining quantities, or atomic weights, we subjoin the following table.

SYMBOLS AND PROPORTIONAL NUMBERS OF THE ELEMENTS.

(From Odling's Manual of Chemistry.)

H	Hydrogen	1	Mg	Magnesium	12
			Zn	Zinc	32.5
Fl	Fluorine	19	Cd	Cadmium	56
Cl	Chlorine	35.5			
Br	Bromine	80	Hg	Mercury, (*Hydrargyrum*)	100
I	Iodine	127	Pb	Lead, (*Plumbum*)	103.5
			Ag	Silver, (*Argentum*)	108
O	Oxygen	16*			
S	Sulphur	32*	Cr	Chromium	26
Se	Selenium	80*	Mn	Maganese	27
T	Tellurium	128*	Fe	Iron, (*Ferrum*)	28
			Ni	Nickel	29
N	Nitrogen	14	Co	Cobalt	30
P	Phosphorous	31	Cu	Copper, (*Cuprum*)	31.7
As	Arsenic	75			
Sb	Antimony, (*Stibium*)	120	Al	Aluminum	13.7
Bi	Bismuth	208	Zr	Zirconium	33.5
			Ce	Cerium	46
C	Carbon	12*	La	Lanthanum	47
Si	Silicon	28.5*	D	Didymium	48
Ti	Titanium	48.5*	U	Uranium	60
Sn	Tin, (*Stannum*)	118*			
Ta	Tantalum	138*	Mo	Molybdenum	48
			Vd	Vanadium	68.5
B	Boron	11	W	Tugsten, (*Wolfram*)	92
Li	Lithium	7	Au	Gold, (*Aurum*)	197
Na	Sodium, (*Natrium*)	23			
K	Potassium, (*Kalium*)	39	Ro	Rhodium	52
			Ru	Ruthenium	52
Ca	Calcium	20	Pa	Palladium	53
Sr	Strontium	41			
Ba	Barium	68.5	Pt	Platinum	98.5
			Ir	Iridium	98.5
G	Glucinum	4.7	Os	Osmium	99.5
Y	Yttrium	32			
Th	Thorium	59.5			

* These elements have had their equivalents doubled to conform to the type theory.

diate constituents. In the above example the 24HO may be driven off by heat, but not by electricity simply; and from other considerations it is impossible to decide from analysis alone whether water is an acid or a base, as it possesses, *according to the substance with which it is combined*, each of these characters; in oil of vitriol it is a base $HO\ SO_3$; in hydrate of potassa, an acid KO HO.

There is still another method of imagining the grouping of the atoms in a complex atom to form a binary compound. This involves the essence of the radical theory.

SO_3 does not redden litmus nor form salts with bases; its compound with HO (oil of vitriol) possesses this property. We may imagine this acid to be HO, SO_3, according to the principles just laid down; or to be $H\ SO_4$, a binary compound, in which H is + and SO_4 is —. If for hydrogen we substitute potassium or any metal, we will have sulphate of potassa or the salt corresponding to the metal. SO_4 is, therefore, a compound radical in the sense in which the word has been employed in chemistry, although it has not been isolated. When water and anhydrous sulphuric acid are brought together, this compound radical is generated by the decomposition of water in the manner illustrated above.

It is, however, more particularly in the case of the bases that the theory of compound radicals has been developed.

The example of ammonia illustrates an inorganic compound radical; if, indeed, it may at present be called inorganic.

The gas ammonia NH_3 (in a manner analogous to that of anhydrous sulphuric acid) acquires basic properties only by the action of water; $NH_3, HO = NH_4\ O$. NH_4 is the compound radical, ammonium. It has never been isolated; it is an hypothetical group of atoms playing the part of a metal.

The following table illustrates the parallelism existing between compound and simple inorganic radicals:

Comp. radical ammonia.		Simple radical potassa.
+ —		+ —
$NH_4 + O$	Oxide.	$K + O$
$NH_4 + S$	Sulphide.	$K + S$
$NH_4 + Cl$	Chloride.	$K + Cl$
$NH_4 + SO_4$	Sulphate.	$K + SO_4$

When organic chemistry began to be developed, the compounds first studied were those containing different proportions of carbon, hydrogen, and oxygen, together with a few containing nitrogen. These were studied in their analogies to inorganic compounds, and the assumption of a large number of organic radicals became imperative. For example, if ether ($C_4\ H_5\ O$) were the oxide of a radical ($C_4\ H_5$) called ethyl, the compounds of ether could be brought into comparison with those of oxides of the different metals, ($C_4\ H_5$,) being a compound organic radical, which group of atoms plays the part of a metal, thus:

Ethyle	(C_4H_5)
Ether	$(C_4H_5)O$
Alcohol	$(C_4H_5)O, HO$
Chloride of ethyle	$(C_4H_5)Cl$
Nitrate of the oxide of ethyle	$(C_4H_5)\ O, NO_5$
Acetate of the oxide of ethyle	$(C_4H_5)O,(C_4H_3)O_3$
Sulphate of the oxide of ethyle	$(C_4H_5)O, SO_3$
Sulphovinic acid	$(C_4H_5)O, SO_3 + HO, SO_3$
Sulphovinate of the oxide of zinc	$(C_4H_5)O, SO_3 + ZnOSO_3$
Potassium	K
Potassa	KO
Hydrate potassa	KO, HO
Chloride potassium	KCl

Nitrate potassa........................	KO, NO_5
Acetate potassa........................	$KO, (C_4H_3)O_3$
Sulphate potassa........................	KO, SO_3
Bi-sulphate potassa........................	$KO,SO_3 + HO, SO_3$
Sulphate potassa and zinc................	$KO, SO_3 + ZnO, SO_3$

Upon this principle, and notwithstanding the fact that for a long time the organic radicals were entirely hypothetical, the development of organic radicals went *pari passu* with the study of organic compounds. The following illustration shows how the organic acids were subjected to the radical theory.

Acetic.........	$(C_2H)O_3$....	with radical	acetyle....	(C_2H)
Propionic	$(C_4H_3)O_3$	"	propyle ...	(C_4H_3)
Butyric.......	$(C_8H_7)O_3$	"	butyle	(C_8H_7)
Valerianic.....	$(C_{10}H_9)O_3$	"	valyle.....	$(C_{10}H_9,)$ &c.

The theory is so simple, so well known, so satisfactory in the explanation of the phenomena to which it is applicable, that the reluctance to abandon it, especially by chemists educated under its influence, is natural. That it has been attacked vigorously, and almost to its fall, is owing to the present great wealth of chemical compounds, and the discovery of phenomena which cannot readily, if at all, be brought in subjection to it.

Daily the realm of chemistry is extending, and the boundary line between organic and inorganic compounds is becoming more and more indistinct. If to the atoms of carbon, hydrogen, oxygen, and nitrogen has been assigned a greater facility of mutual chemical attraction, the reason lies less, perhaps, in a peculiarity of the nature of these atoms, than in the kind of experiments to which they have been subjected. Continually, elements formerly called inorganic are added to organic compounds, and it is not too much to expect that the same chemical attractions exist between all of the elements as between C, H, O, and N *inter illis*. If the right of combining, in indefinite number of atoms, the original organic elements, gives rise to so many "*changes*," *i. e.*, compounds, what would it be if each of the sixty-four elements could play an equal part with these?

The number of possible chemical compounds would approach infinity, and could only be conceived by the aid of comparison. It would be no exaggeration to compare their number with the distance from the earth of the fixed stars expressed in feet, or even with the diameter of that great orbit in which our solar system is supposed to be moving.

It is true that theories are not formed to meet *future* wants; but, nevertheless, a general consideration that the radical theory was becoming daily insufficient for the rapid increase of chemical facts, urged thoughtful men to invent a theory which should, at least, generalize chemical compounds, and bring them into the proper order and connexion to render their more perfect study possible. A satisfactory theory has not yet been invented, and chemists are loath to abandon totally the electro-radical theory for that of types pure and simple.

While the radical theory was in a very flourishing condition, certain newly observed phenomena demonstrated that we could substitute electro-negative chlorine for electro-positive hydrogen in a compound without changing the chemical character of the body to a great extent. Thus, by the action of chlorine (Cl) upon olefiant gas, (C_4 H_4,) four Dutch chemists had many years ago discovered a peculiar compound, which has received the name of Dutch liquid, and which has the composition C_4 H_4 Cl_2. When upon this body the action of chlorine was continued, supported by sun light, it was discovered that a series of liquids could be obtained having the same character as Dutch liquid, but differing in that the hydrogen was replaced atom by atom by chlorine, thus:

$$\begin{array}{llcl}
\text{Dutch liquid} & C_4H_4Cl_2 & = & (C_4H_4)Cl_2 \\
\text{1st substitution} & C_4H_3Cl_3 & = & \left(C_4\left\{\begin{matrix}H_3\\Cl\end{matrix}\right.\right)Cl_2 \\
\text{2d} \quad `` & C_4H_2Cl_4 & = & \left(C_4\left\{\begin{matrix}H_2\\Cl_2\end{matrix}\right.\right)Cl_2 \\
\text{3d} \quad `` & C_4H\,Cl_5 & = & \left(C_4\left\{\begin{matrix}H\\Cl_3\end{matrix}\right.\right)Cl_2 \\
\text{4th} \quad `` & C_4Cl_6 & = & (C_4Cl_4)Cl_2
\end{array}$$

If upon the members of this series an alcoholic solution of potassa act, one equivalent each of hydrogen and chlorine is separated, and we obtain the following compounds:

$$\begin{array}{lllcl}
\text{From Dutch liquid} & C_4H_4Cl_2 & \text{we obtain} & C_4\begin{matrix}H_3\\Cl\end{matrix}\Big\} \\
``\ \text{1st substitution} & C_4H_3Cl_3 & `` & C_4\begin{matrix}H_2\\Cl_2\end{matrix}\Big\} \\
``\ \text{2d} \quad `` & C_4H_2Cl_4 & `` & C_4\begin{matrix}H\\Cl_3\end{matrix}\Big\} \\
``\ \text{3d} \quad `` & C_4HCl_5 & `` & C_4Cl_4;
\end{array}$$

which demonstrates that in Dutch liquid and its chlorine compounds the latter element exists in two conditions: one in which it takes the place of hydrogen, atom by atom, and another in which it unites with carbon and the compound atom thus formed. In other words, the negative atom of chlorine drives out and takes the place of the positive atom of hydrogen. To bring these phenomena in accord with the former electro-chemical theory, we would have to assign to the atom of chlorine a preponderating positive and a negative character at the same time, which was deemed inadmissible.

The same difficulty occurred with respect to the *negative* atom oxygen, to which, according to some, a place had to be assigned sometimes inside of the *positive* radical.

The behavior of acetic acid with chlorine gas in sun-light affords a striking example of the substitution of Cl for H. By this reaction, from $C_4\,H_4\,O_4$ (acetic acid) there arises, by the substitution of chlorine for hydrogen, chloracetic acid, $\left(C_4\begin{matrix}H\\Cl_3\end{matrix}\Big\}O_4,\right)$ and between the two acids there is a great chemical similarity. They each saturate the same amount of base, and when acted upon by the same reagents, give rise to analogous products. Thus, by heating with excess of alkali, acetic acid becomes carbonic acid ($2\,C\,O_2$) and light carburetted hydrogen, ($C_2\,H_4$,) while by the same treatment chloracetic acid becomes $2\,C\,O_2$ and $C_2\begin{matrix}H\\Cl_3,\end{matrix}\Big\}$ or chloroform, which may be regarded as light carburetted hydrogen, in which a portion of the hydrogen is replaced by chlorine. By the action of nascent hydrogen, chloracetic acid is regenerated to acetic acid. It is true that these difficulties might be reconciled by the assumption of both a negative and positive character being assumed under different circumstances by the same atom. This must be done in certain instances to bring the modern type theory in accord with the electro-chemical theory, and, indeed, the experiments of Schoenbein upon ozone, and the phenomena of the action of certain bodies in the "nascent" state, would render this assumption not unlikely; but the immediate result of the experiments cited was to hold the electro-chemical theory in abeyance, and to develop the theory of types.

The first type theory was a theory of the *classification* of chemical compounds, and was analogous to the natural history system of classification into orders, genera, and species. There was a "molecular" or "mechanical" type

which corresponded to the "order;" a "chemical" type to the "genus;" and the various members under the same chemical type corresponded to the "species."

Compounds of the same molecular type consisted of the same number of atoms; but not in binary groups, as the electro-chemical theory required.

Under each molecular type were the chemical types, consisting of the same number of atoms (as before) but similarly arranged. The individuals of the same chemical type consisted of the same number of atoms, similarly arranged, but differing in the kind of atoms. The following example will illustrate the theory:

MOLECULAR TYPE OF TWELVE ATOMS.

1st chemical type	Acetic acid.....	$C_4 H_4 O_4$	Individuals of 1st chemical type.
	Chloracetic acid.	$C_4 Cl_3 HO_4$	
2d chemical type	Alcohol	$C_4 H_6 O_2$	Individuals of 2d chemical type.
	Mercaptan.....	$C_4 H_6 S_2$	

These all belong to the same molecular type of twelve atoms. The first two and the last two belong, respectively, to the same *chemical* type; the atoms are regarded as being similarly arranged, because acetic and chloracetic acids, on the one side, and alcohol and mercaptan on the other, bear a great analogy to each other in their compounds and in the products of their decomposition by the same reagents. The following method was adopted for writing the formulæ according to this theory:

Acetic acid	$C_4 \left.\begin{matrix} H_3 \\ H \end{matrix}\right\} O_4$
Chloracetic acid............	$C_4 \left.\begin{matrix} Cl_3 \\ H \end{matrix}\right\} O_4$
Acetate of potassa..........	$C_4 \left.\begin{matrix} H_3 \\ K \end{matrix}\right\} O_4$
Acetic ether	$C_4 \left.\begin{matrix} H_3 \\ (C_4H_5) \end{matrix}\right\} O_4$
Chloracetic ether	$C_4 \left.\begin{matrix} Cl_3 \\ (C_4H_5) \end{matrix}\right\} O_4$

The following contain $C_8 H_8 O_4$, but the atoms are arranged differently

Butyric acid	$C_8 \left.\begin{matrix} H_7 \\ H \end{matrix}\right\} O_4$
Acetic ether	$C_4 \left.\begin{matrix} H_3 \\ (C_4H_5) \end{matrix}\right\} O_4$
Propionate of methyle.......	$C_6 \left.\begin{matrix} H_5 \\ (C_2H_3) \end{matrix}\right\} O_4$

It will be observed that chlorine, in the type, takes the place of the *upper* hydrogen atoms and potassium, and the radicals the place of the *lower* ones, thus indicating the different nature of the several hydrogen atoms in the type; and, further, that this theory was obliged to assent to the idea of "radicals," namely, groups of atoms playing the part of single atoms.

The type theory met with many supporters, some of them the best thinkers which have enriched modern chemistry; it met with many variations, some of which penetrated far into the realms of fancy; but it would probably have fallen into disuse had not the discovery of the compound ammonias directed the attention of the chemical world to this method of imagining the constitution of chemical compounds.

At the same time that attention to this subject was arrested, homologous series were discovered, (by a type theorist,) and important laws with respect to them, such as the relative boiling points of their members, their vapor den-

sity, atomic volume, &c., became known; out of which accessions to our knowledge was developed the modern type theory.

The compound ammonias are bases bearing a very great analogy to ammonia, their salts being strictly analogous. By the former radical theory it would be impossible to assign to them satisfactory formulæ; but by the assumption that they are constituted after the pattern or type of ammonia, their formulæ become very simple. They are ammonias, in which one or more atoms of hydrogen are replaced by one or more radicals, thus:

$$\text{Type } \left.\begin{matrix} H \\ H \\ H \end{matrix}\right\} N = \text{Ammonia.}$$

Ethylamine.	Diethylamine.	Triethylamine.
$\left.\begin{matrix} (C_4H_5) \\ H \\ H \end{matrix}\right\} N.$	$\left.\begin{matrix} (C_4H_5) \\ (C_4H_5) \\ H \end{matrix}\right\} N.$	$\left.\begin{matrix} (C_4H_5) \\ (C_4H_5) \\ (C_4H_5) \end{matrix}\right\} N.$

The laws alluded to above which enable a more correct conception of the chemical constitution of bodies are as follows:

1. *The law of even atoms.*—The remarkable fact has been discovered that (the eqivalents of O and H being 8 and 1) by far the greatest number of organic compounds contain an *even* number of carbon atoms; further, that the sum of the atoms of hydrogen, chlorine, iodine, bromine, nitrous oxide, ($N\,O_4$,) nitrogen, and metal is an even number; which is also true for the sum of their oxygen and sulphur-atoms. For example, in Benzoic acid $C_{14}\,H_6\,O_4$ the number of carbon atoms is an even number, and so is that of the hydrogen and of the oxygen atoms.

2. *The law of atomic volume.*—The greater portion of organic compounds experience in the vaporous condition a condensation during the combination of their elements to four volumes—in other words, in the state of vapor their atom occupies four times the volume of an oxygen atom. This law, it will be remembered, is seen by comparing the quotients arising from a division of the equivalents of compounds by the specific gravity of their vapors, and gives the result that the atomic volume of the atoms of the elements and their compounds bear a simple relation to each other, as may be seen from the following table, which is quoted from its bearing upon the type theory:

Names of bodies.	Symbol.	Division of the equiv. by the sp. gr. of vapor.	Relative atomic volume.	Atomic volume. Oxygen = 1.
Sulphur	S	$\frac{16}{6639}$	2.41	$\frac{1}{2}$
Oxygen	O	$\frac{8}{1108}$	7.22	1
Phosphorus	P	$\frac{31}{4294}$	7.22	2
Hydrogen	H	$\frac{1}{00693}$	14.44	2
Nitrogen	N	$\frac{14}{0969}$	14.44	2
Chlorine	Cl	$\frac{355}{2458}$	14.44	2
Bromine	Br	$\frac{80}{554}$	14.44	2
Iodine	I	$\frac{1271}{8802}$	14.44	2
Water	HO	$\frac{9}{0623}$	14.44	2
Sulphuretted hydrogen	HS	$\frac{17}{1177}$	14.44	2
Carbonic acid	CO_2	$\frac{22}{1524}$	14.44	2
Protoxide of nitrogen	NO	$\frac{22}{1524}$	14.44	2
Deutoxide of nitrogen	NO_2	$\frac{30}{11039}$	28.88	4
Hydrochloric acid	HCl	$\frac{365}{1264}$	28.88	4
Ammonia	NH_3	$\frac{17}{0589}$	28.88	4
Chloride of ethyle	$C_4\,H_5\,Cl$	$\frac{645}{2233}$	28.88	4
Acetic acid	$C_4\,H_4\,O_4$	$\frac{60}{2078}$	28.83	4
Valerianate of ethyle	$C_{14}\,H_{14}\,O_4$	$\frac{130}{4501}$	28.88	4

So closely do chemical compounds conform to this law that it is used daily to control vapor density determinations; the experiments show whether the condensation is to 1, 2, or 4 volumes, and whether, accordingly, the equivalent of the body is to be divided by 7.22, 14.44, or 28.88, to calculate the density of its vapor. The calculation is more accurate than the actual experiment on account of the superior accuracy by which the equivalents have been determined. The law of even atoms, and the observation that in most organic compounds the condensation is to 4 volumes, serve often to determine the formulæ of organic compounds. Thus, to acetone was formerly assigned the formula $C_3 H_3 O$, which satisfies neither law; by doubling its formula (and there is no chemical reason to the contrary) it becomes $C_6 H_6 O_2$, which satisfies both laws. For the same reason the formula of ether ($C_4 H_5 O$) may be doubled to $C_8 H_{10} O_2$.

Again: it has been doubted from its origin and chemical behavior whether amyle obtained from amylic alcohol ($C_{10} H_{12} O_2$) should have the formula $C_{10} H_{11}$, or $C_{20} H_{22}$; but the latter formula agrees with the law of even atoms, and with a condensation to four volumes.

3. *The law of homologous series.*—Another law influencing strongly the determination of chemical formulæ, and which is one of the most remarkable among the discoveries of modern chemistry, is that of homologous series.

The following is an example:

SERIES ($C_n H_{n \times 2}$.)

Bodies.	Formulæ.	Boiling point.	Sp. gr. at 0° C.	Sp. gr. of vapor.
Ethyle butyle	$\left\{ \begin{matrix} C_4 H_5 \\ C_8 H_9 \end{matrix} \right\} = C_{12} H_{14}$	62° C.	0.701	2.97
Ethyle amyle	$\left\{ \begin{matrix} C_4 H_5 \\ C_{10} H_{11} \end{matrix} \right\} = C_{14} H_{16}$	85°	0.707	3.46
Butyle	$\left\{ \begin{matrix} C_8 H_9 \\ C_8 H_9 \end{matrix} \right\} = C_{16} H_{18}$	108°	0.716	3.94
Butyle amyle	$\left\{ \begin{matrix} C_8 H_9 \\ C_{10} H_{11} \end{matrix} \right\} = C_{18} H_{20}$	132°	0.725	4.42
Amyle	$\left\{ \begin{matrix} C_{10} H_{11} \\ C_{10} H_{11} \end{matrix} \right\} = C_{20} H_{22}$	158°	0.741	4.91

The members of this series are subject to the "*same law;*" they advance from the lowest by an increment of $C_2 H_2$. A general formula for the series would be $C_n H_{(n+2)}$, n being an even whole number. Their boiling points as well as their specific gravities in the liquid and in the vaporous condition rise gradually. We have, from its position in this series, an additional reason why amyle should have the formula $C_{20} H_{22}$, and not $C_{10} H_{11}$. Indeed, as may be seen in the table, amyle is regarded as having (in combination) $C_{10} H_{11}$, but, when *in the free state*, two of its atoms are joined together to form a compound atom $C_{20} H_{22}$. The following are additional illustrations of homology

I. HYDROCARBONS.

($C_n H_n$.)

Ethylene	$C_4 H_4$
Propylene	$C_6 H_6$
Butylene	$C_8 H_8$
Amylene	$C_{10} H_{10}$
Oleëne	$C_{12} H_{12}$

II. ACIDS.

$C_n H_n O_4$.

Formic	$C_2 H_2 O_4$
Acetic	$C_4 H_4 O_4$
Propionic	$C_6 H_6 O_4$
Butyric	$C_8 H_8 O_4$
Valerianic	$C_{10} H_{10} O_4$
....	
Palmitic	$C_{32} H_{32} O_4$
Stearic	$C_{36} H_{36} O_4$

III. ALCOHOLS $C_n H_{(n+2)} O_2$		IV. BASES. $C_n H_{(n-5)} N$		V. HYDROCARBONS. $C_n H_{(n-6)}$	
Formylic	$C_2 H_4 O_2$	Aniline.....	$C_{12} H_7 N$	Benzole	$C_{12} H_6$
Ethylic	$C_4 H_6 O_2$	Toluidine...	$C_{14} H_9 N$	Toluole	$C_{14} H_8$
Propylic	$C_6 H_8 O_2$	Xylidine ...	$C_{16} H_{11} N$	Xylole........	$C_{16} H_{10}$
Butylic	$C_8 H_{10} O_2$	Cumidine...	$C_{18} H_{13} N$	Cumole	$C_{18} H_{12}$
Amylic	$C_{10} H_{12} O_2$	Cymidine...	$C_{20} H_{15} N$	Cymole	$C_{20} H_{14}$
....	....				
Aethalic	$C_{32} H_{34} O_2$				

The most remarkable phenomenon connected with homologous series is not the uniform law according to which the formulæ are developed; but that the successive increment of the atoms $C_2 H_2$ contributes to a certain regularity of physical and chemical character; thus, neighbors in the series have greater analogy to each other than to more distant members. The acids and alcohols quoted advance (at the normal temperature) by degrees from liquids to solids; and chemically, formic and acetic acids on the one hand, and palmitic and stearic acids on the other are analogous. The boiling points increase with regularity; for example, in series II and III every addition of $C_2 H_2$ adds 19° C. to the boiling point. Though this regularity of boiling point applies to other series, the difference is not the same for all; thus in series V every increment of $C_2 H_2$ adds 24° C. to the boiling point.

It would create too great a diversion from the main object of the present article to enter further upon the nature of homologous series. The curious law may, however, be cited with respect to certain series of acids, ethers, and alcohols, viz: that if two of them have an equal number of hydrogen and oxygen atoms, and one has X more atoms of carbon, the latter will boil at X 14.5 degrees centigrade higher. For example:

Benzoic acid,	$C_{14} H_6 O_4$;	boiling point,	253°
Propionic acid,	$C_6 H_6 O_4$;	"	137
Difference,	C_8	$8 \times 14.5 =$	116°
Angelica acid,	$C_{10} H_8 O_4$;	boiling point,	185°
Butyric acid,	$C_8 H_8 O_4$;	"	156°
Difference,	C_2	$2 \times 14.5 =$	29°

On the other hand, if the number of atoms of carbon and oxygen is the same, and one compound contains X equivalents less of hydrogen, its boiling point will be X 5 C.° higher.

Angelica acid,	$C_{10} H_8 O_4$;	boiling point,	185°
Valerianic acid,	$C_{10} H_{10} O_4$;	"	175°
Difference,	H_2	$2 \times 5 =$	10°

Not only are the members of the same series subjected to one and the same law, but some of the series are connected with each other. The importance of this fact is very great, since it enables a systematic grouping of chemical compounds. From the character of well-studied bodies, and from the analogies alluded to, we are able to pronounce a judgment upon the chemical constitution, nature, and behavior of new bodies.

This connexion of the series is as follows:

From alcohol $C_4 H_6 O_2$ we may obtain by the addition of oxygen, and by the subtraction of hydrogen, acetic acid, $(C_4 H_6 O_2) + O_4 = (C_4 H_4 O_4) + 2 H O$. Hence, in general terms, if from the series $(C_n H_{(n+2)} O_2)$ we subtract H_2, and add $O_2 = (C_n H_n O_4,)$ we obtain an acid analogous to acetic acid. Moreover,

by subtracting 2 H O from (C_n $H_{(n+2)}$ O_2) we obtain C_n H_n, or the series of hydrocarbons (I.) Ethylene is thus actually obtained from alcohol.

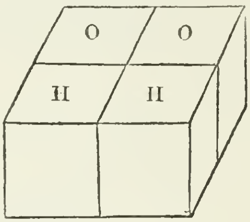
Again, from every acid of the series C_n H_n O_4 we may obtain an amide C_n $H_{(n+1)}$ N O_2.

The law of homology conduced strongly to the type theory by contributing a better knowledge of the chemical constitution of bodies. By its study, radicals containing oxygen were definitely accepted. Thus, (in series II,) acetic acid (C_4 H_4 O_4) is not formed from ethylene (C_4 H_4) by the addition of O_4, but from alcohol (C_4 H_6 O_2) by the addition of O_2, and by the subtraction of H_2 in such manner that the radical (C_4 H_3 O_2) is formed; which makes acetic acid (C_4 H_4 O_4) = (C_4 H_3 O_2) H O_2. As a proof of the existence of such a radical in acetic acid, we may obtain its compound with chlorine by the action of oxychloride of phosphorus upon acetate of soda, and we may restore this chlorine compound to acetic acid by the action of water upon it.*

(NO_4) is another radical containing oxygen. By acting upon benzoic acid so as to substitute (NO_4) for hydrogen, we have nitro-benzoic acid—that is, C_{14} H_6 O_4 becomes C_{14} H_5 (NO_4) O_4.

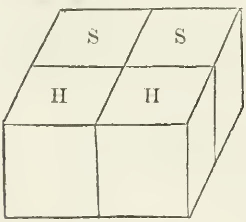
These considerations have been leading us gradually to the ideas of modern chemical types. Such a type is a group of atoms of which the individuals bear a certain relation to each other, and forms a pattern for imagining all chemical compounds, between the atoms of which a similar relation is supposed to exist. It may be illustrated by certain blocks glued together, or by a cage of wire with compartments, in which the blocks may be placed, thus:

THE TYPE—WATER.

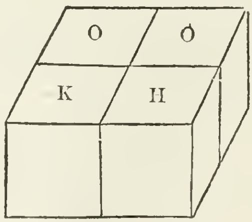


Examples of the type of water:

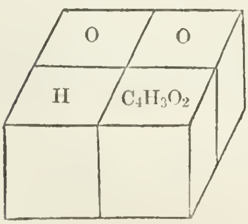
Sulphuretted Hydrogen.



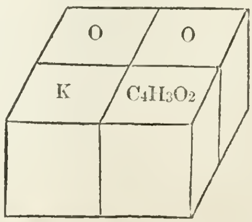
Hydrate of Potassa.



Acetic Acid.



Acetate of Potassa.



* See examples of reactions by the type method toward the close of this article.

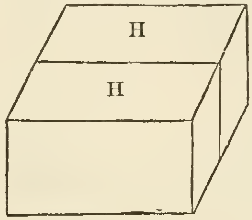
The four compounds represented above are supposed to be constituted after the pattern of water, which is the type. Thus, if the oxygen atoms of the type are replaced by sulphur atoms, we have sulphuretted hydrogen.

If, in the type, we substitute for the hydrogen block, upon the left hand, a potassium block, the result is hydrate of potassa. If, on the other hand, we remove a right-hand block of hydrogen, and substitute a block representing the radical acetyle, we have acetic acid. And if we replace each hydrogen block of the type, one with a potassium block and the other with an acetyle block, there results acetate of potassa.

In the above illustration the compounded blocks are of one size, thus representing a volume of four oxygen blocks, and conforming to the law of condensation of organic compounds to four volumes. We must bear in mind that the individual blocks may be larger than an oxygen block when outside of the type, though condensed to the size of such block in the compound. For example, 2 volumes of oxygen + 4 volumes of hydrogen = 6 volumes, which are condensed, by combination of the gases, to 4 volumes of vapor of water.

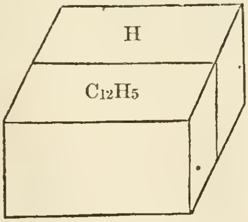
Hydrogen constitutes another type, thus:

HYDROGEN TYPE.

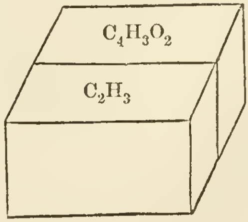


Benzole and Acetone examples of this type are represented thus:

Benzole.

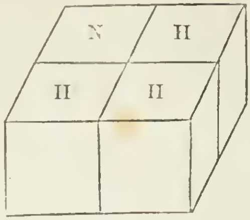


Acetone.



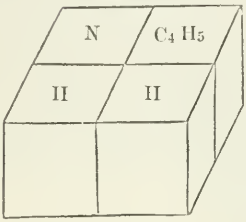
Ammonia gives another type, thus:

AMMONIA TYPE.

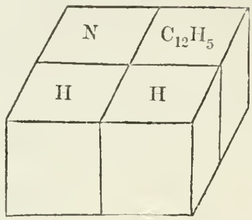


Ethylamine and Aniline are examples of this type, and are thus represented:

Ethylamine.



Aniline.



The *sizes* of these hydrogen and ammonia types are equal to that of the water type, viz: four volumes.

With this preliminary illustration the following table (from Graham Otto's Lehrbuch) may be quoted:

COMPOUNDS ACCORDING TO THE TYPE WATER $= H_2\ O_2 = \left.\begin{matrix}H\\H\end{matrix}\right\}O_2.$

$\left.\begin{matrix}H\\K\end{matrix}\right\}O_2.$ Hydrate potassa.	$\left.\begin{matrix}H\\C_2\ H_3\end{matrix}\right\}O_2.$ Wood spirit.	$\left.\begin{matrix}H\\C_{12}\ H_5\end{matrix}\right\}O_2.$ Phenole.	$\left.\begin{matrix}C_4\ H_3\ O_2\\NH_4\end{matrix}\right\}O_2.$ Acetate ammonia.	$\left.\begin{matrix}C_4\ H_3\ O_2\\K\end{matrix}\right\}O_2.$ Acetate potassa.	$\left.\begin{matrix}NO_4\\K\end{matrix}\right\}O_2.$ Nitrate potassa.	$\left.\begin{matrix}C_2\ HO_2\\H\end{matrix}\right\}O_2.$ Formic acid.	$\left.\begin{matrix}NO_4\\H\end{matrix}\right\}O_2.$ Nitric acid.
$\left.\begin{matrix}H\\NH_4\end{matrix}\right\}O_2.$ Hydrate oxide of ammonium.	$\left.\begin{matrix}H\\C_4\ H_5\end{matrix}\right\}O_2.$ Alcohol.	$\left.\begin{matrix}C_2\ H_3\\C_4\ H_5\end{matrix}\right\}O_2.$ Methyle ethyle ether.	$\left.\begin{matrix}C_4\ H_3\ O_2\\C_2\ H_3\end{matrix}\right\}O_2.$ Acetate of oxide of methyle.	$\left.\begin{matrix}C_{14}\ H_5\ O_2\\C_4\ H_5\end{matrix}\right\}O_2.$ Benzoate of oxide of ethyle.	$\left.\begin{matrix}NO_4\\C_4\ H_5\end{matrix}\right\}O_2.$ Nitrate of oxide of ethyle.	$\left.\begin{matrix}C_4\ H_3\ O_2\\H\end{matrix}\right\}O_2.$ Acetic acid.	$\left.\begin{matrix}C_4\ H_3\ O_2\\H\end{matrix}\right\}O_2.$ Chloracetic acid.
$\left.\begin{matrix}K\\K\end{matrix}\right\}O_2.$ Potassa.	$\left.\begin{matrix}K\\C_4\ H_5\end{matrix}\right\}O_2.$ Potassa ether.	$\left.\begin{matrix}C_4\ H_5\\C_4\ H_5\end{matrix}\right\}O_2.$ Ether.	$\left.\begin{matrix}C_4\ H_3\ O_2\\C_4\ H_5\end{matrix}\right\}O_2.$ Acetate of oxide of ethyle.	$\left.\begin{matrix}C_4\ Cl_3\ O_2\\C_4\ H_5\end{matrix}\right\}O_2.$ Chloracetate of oxide of ethyle.	$\left.\begin{matrix}C_2\ N\\C_4\ H_5\end{matrix}\right\}O_2.$ Cyanate of oxide of ethyle.	$\left.\begin{matrix}C_4\ H_3\ O_2\\C_4\ H_3\ O_2\end{matrix}\right\}O_2.$ Anhydrous acetic acid.	$\left.\begin{matrix}NO_4\\NO_4\end{matrix}\right\}O_2.$ Anhydrous nitric acid.

COMPOUNDS ACCORDING TO THE TYPE HYDROGEN $H_2 = \left.\begin{matrix}H\\H\end{matrix}\right\}$

$\left.\begin{matrix}H\\C_2\ H_3\end{matrix}\right\}$ Methyle hydrogen.	$\left.\begin{matrix}H\\C_{12}\ H_5\end{matrix}\right\}$ Benzole.	$\left.\begin{matrix}C_4\ H_5\\C_4\ H_5\end{matrix}\right\}$ Ethyle.	$\left.\begin{matrix}C_4\ H_3\ O_2\\C_2\ H_3\end{matrix}\right\}$ Acetone.	$\left.\begin{matrix}Cl\\C_4\ H_5\end{matrix}\right\}$ Chloride of ethyle.	$\left.\begin{matrix}C_2\ N\\C_4\ H_5\end{matrix}\right\}$ Cyanide of ethyle.	$\left.\begin{matrix}C_4\ H_3\ O_2\\H\end{matrix}\right\}$ Aldehyde.	$\left.\begin{matrix}Cl\\H\end{matrix}\right\}$ Hydrochloric acid.	$\left.\begin{matrix}Cl\\C_4\ H_3\ O_2\end{matrix}\right\}$ Chloracetic acid.	$\left.\begin{matrix}Cl\\Cl\end{matrix}\right\}$ Chlorine.

COMPOUNDS ACCORDING TO THE TYPE AMMONIA $H_3\ N = \left.\begin{matrix}H\\H\\H\end{matrix}\right\}N.$

$\left.\begin{matrix}C_{12}\ H_5\\H\\H\end{matrix}\right\}N.$ Aniline.	$\left.\begin{matrix}C_{12}\ H_4\ (NO_4)\\H\\H\end{matrix}\right\}N.$ Nitraniline.	$\left.\begin{matrix}C_4\ H_5\\H\\H\end{matrix}\right\}N.$ Ethylamine.	$\left.\begin{matrix}C_4\ H_5\\C_4\ H_5\\H\end{matrix}\right\}N.$ Di-ethylamine.	$\left.\begin{matrix}C_4\ H_5\\C_4\ H_5\\C_4\ H_5\end{matrix}\right\}N.$ Tri-ethylamine.	$\left.\begin{matrix}C_{12}\ H_5\\C_{14}\ H_5\ O_2\\C_{14}\ H_5\ O_2\end{matrix}\right\}N.$ Di-benzoyl-phenylamide.	$\left.\begin{matrix}C_4\ H_3\ O_2\\H\\H\end{matrix}\right\}N.$ Acetamide.	$\left.\begin{matrix}C_{14}\ H_5\ O_2\\H\\H\end{matrix}\right\}N.$ Benzamide.

The table illustrates the method of writing the formulæ of bodies according to the types of water, hydrogen, and ammonia, to which they respectively belong. Determinations of the specific gravities of the vapor of water and of hydrogen show that the formulæ $H\,O$ and H ($O = 8$ and $H = 1$) agree to a condensation of *two* volumes. In order, therefore, to make types of these bodies, their formulæ must be doubled so as to correspond to a condensation of four volumes, which is the atomic volume of the greater part of organic compounds.

The formula for ammonia $N\,H_3$ already corresponds to four volumes, *e. g.*, 2 vols. $N + 6$ vols. $H = 8$ vols. condensed to 4 vols. $N\,H_3$.

It will be observed that, in the table, compounds of a basic character are placed to the left hand, those of acid nature to the right hand, while salt or neutral bodies occupy positions in the middle of the table.

It will be observed, further, that in the formulæ of the bodies according to the types $\left.\begin{matrix} H \\ H \end{matrix}\right\} O_2$ and $\left.\begin{matrix} H \\ H \end{matrix}\right\}$ the electro-negative elements are placed in the bracket to the left hand, and these are distinguished into a superior atom of hydrogen, capable of being replaced by chlorine, &c., or an acid radical, and an inferior atom of hydrogen susceptible of being exchanged for a metal or basic radical, while the electro-negative elements, oxygen, sulphur, &c., occupy positions to the right hand, outside of the bracket.

The relations existing between anhydrous or hydrated acids or bases; the difference between hydrogen acids and oxygen acids; the nature of acid, base, or salt, are more readily perceived by a close examination of the table than by the most extended description. It will be seen by this inspection how the ammonia salts are represented. $\left.\begin{matrix} C_4\,H_3\,O_2 \\ N\,H_4 \end{matrix}\right\} O_2$ is the acetate of ammonia. By adding H to the type ammonia we have a new type $\left.\begin{matrix} H \\ H \\ H \\ H \end{matrix}\right\} N$, ammonium, which enables the formation of salts, according to the ammonium theory, by introducing this new type into the type of water. Thus diethyle-methyle-amyle-ammonium would be

$$\left.\begin{matrix} C_4\,H_5 \\ C_4\,H_4 \\ C_2\,H_3 \\ C_{10}\,H_{11} \end{matrix}\right\} N. \text{ Its hydrated oxide } \left.\begin{matrix} H \\ \left.\begin{matrix} C_4\,H_5 \\ C_4\,H_5 \\ C_2\,H_3 \\ C_{10}\,H_{11} \end{matrix}\right\} N. \end{matrix}\right\} O_2. \text{ Its acetate } \left.\begin{matrix} C_4\,H_3\,O_2 \\ \left.\begin{matrix} C_4\,H_5 \\ C_4\,H_5 \\ C_2\,H_3 \\ C_{10}\,H_{11} \end{matrix}\right\} N. \end{matrix}\right\} O_2.$$

The homologous series may thus be generalized by this system of nomenclature—*e. g.*, ordinary alcohol is $\left.\begin{matrix} H \\ C_4\,H_5 \end{matrix}\right\} O_2$; $\left.\begin{matrix} H \\ C_n\ \ H_{(n+1)} \end{matrix}\right\} O_2$ is any alcohol, and $\left.\begin{matrix} C_n\,H_{(n-1)}\ O_2 \\ H \end{matrix}\right\}$ any corresponding acid of the same homologous series.

Another principle, which has been adopted in the type method, consists in the assumption of radicals capable of replacing H_2 or H_3 in the types. Such radicals are diatomic when they replace H_2, and are represented thus, ($''$,) and triatomic ($'''$) when they replace H_3; and the types of water, hydrogen, and ammonia are doubled or trebled to form new types by which bibasic or tribasic acids or salts may be represented. Thus:

BIBASIC ACIDS.

Type.	Sulphuric acid.	Succinic acid.	Tartaric acid.
$\left.\begin{matrix} H_2 \\ H_2 \end{matrix}\right\} O_4.$	$\left.\begin{matrix} S_2\,O_4'' \\ H_2 \end{matrix}\right\} O_4.$	$\left.\begin{matrix} C_8\,H_4\,O_4'' \\ H_2 \end{matrix}\right\} O_4.$	$\left.\begin{matrix} C_8\,H_4\,O_8'' \\ H_2 \end{matrix}\right\} O_4.$

TRIBASIC ACIDS.

Type.	Phosphoric.	Meconic.	Citric.
$\left.\begin{matrix} H_3 \\ H_3 \end{matrix}\right\} O_6.$	$\left.\begin{matrix} P\,O_2''' \\ H_3 \end{matrix}\right\} O_6.$	$\left.\begin{matrix} C_{14}\,H\,O_8''' \\ H_3 \end{matrix}\right\} O_6.$	$\left.\begin{matrix} C_{12}\,H_5\,O_8''' \\ H_3 \end{matrix}\right\} O_6.$

The following are examples of the duplication and triplication of the hydrogen and ammonium types:

Type. Chloro-sulphuric acid. Chloride of succinyle.

$$\left.\begin{matrix}H_2\\H_2\end{matrix}\right\}\qquad\left.\begin{matrix}S_2\,O_4''\\Cl_2\end{matrix}\right\}\qquad\left.\begin{matrix}C_8\,H_4\,O_4''\\Cl_2\end{matrix}\right\}$$

Type. Succinimide.

$$\left.\begin{matrix}H_2\\H_2\\H_2\end{matrix}\right\}N_2.\qquad\left.\begin{matrix}C_8\,H_4\,O_4''\\H_2\\H_2\end{matrix}\right\}N_2.$$

Type. Oxychloride of phosphorus.

$$\left.\begin{matrix}H_3\\H_3\end{matrix}\right\}\qquad\left.\begin{matrix}P\,O_2'''\\Cl_3\end{matrix}\right\}$$

Type. Citramide.

$$\left.\begin{matrix}H_3\\H_3\\H_3\end{matrix}\right\}N_3.\qquad\left.\begin{matrix}C_{12}\,H_5\,O_8'''\\H_3\\H_3\end{matrix}\right\}N_3.$$

These derived types are connected with the primary types by the hypothesis that a "polyatomic" radical may replace *several* atoms of hydrogen in the *primary* type. Thus—

Type. Anhydrous sulphuric acid. Anhydrous succinic acid.

$$\left.\begin{matrix}H\\H\end{matrix}\right\}O_2\qquad S_2\,O_4''\}\,O_2.\qquad C_8\,H_4\,O_4''\}\,O_2.$$

Type. Sulphurous acid.

$$\left.\begin{matrix}H\\H\end{matrix}\right\}\qquad S_2\,O_4''\}$$

Type. Succinimide.

$$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N.\qquad\left.\begin{matrix}C_8\,H_4\,O_4''\\H\end{matrix}\right\}N.$$

The following examples illustrate the use of the type method of expressing a chemical reaction—*e. g.*, that of hydrochloric acid with hydrated oxide of ammonium.

By the former method it would be—

$$N\,H_4\,O + H\,Cl = N\,H_4\,Cl + H\,O$$

By the type method—

$$\left.\begin{matrix}H\\N\,H_4\end{matrix}\right\}O_2+\left.\begin{matrix}Cl\\H\end{matrix}\right\}=\left.\begin{matrix}Cl\\N\,H_4\end{matrix}\right\}+\left.\begin{matrix}H\\H\end{matrix}\right\}O_2$$

Again: by the action of oxychloride of phosphorus upon acetate of soda, chloride of acetyle is formed together with phosphate of soda, which reaction is represented.

By the former method—

$$P\,O_2\,Cl_3 + 3\,(Na\,O,\,C_4\,H_3\,O_3) = 3\,NaO,\,P\,O_5 + 3\,(C_4\,H_3\,O_2)\,Cl.$$

By the type method—

$$\left.\begin{matrix}P\,O_2'''\\Cl_3\end{matrix}\right\}+3\left(\left.\begin{matrix}C_4\,H_3\,O_2\\Na\end{matrix}\right\}O_2\right)=\left.\begin{matrix}P\,O_2'''\\Na_3\end{matrix}\right\}O_6+3\left\{\begin{matrix}C_4\,H_3\,O_2\\Cl.\end{matrix}\right.$$

Some regard the type method of imagining a body as essential in the nature of matter; to these the type of the same body is invariable, with which, if

phenomena agree not, the reason is sought, and the correct type determined by experiment.

But others employ this method as a means of comparing chemical reactions, and as suggestive of new experiments. Such write formulæ sometimes according to the old nomenclature, and sometimes take great liberties with the types, viewing the same body in different types; for example, taking aldehyde ($C_4\ H_4\ O_2$) according to the type $\frac{H}{H}$, thus: $\left.\begin{matrix} C_4\ H_3\,O_2 \\ H \end{matrix}\right\}$ or, according to the type $\left.\begin{matrix} H \\ H \end{matrix}\right\} O_2$, thus: $\left.\begin{matrix} C_4\ H_3 \\ H \end{matrix}\right\} O_2$.

A very serious defect, in my opinion, in the type method is that it places the hydrogen acids and salts in a different type from the oxygen acids and salts; while the analogies existing between these acids and salts furnish urgent reason why they should have the same constitution, which similarity chemists have always labored to discover. It is not fair to constitute a type ammonia founded on the chemical analogies of it and the compound ammonias, and at the same time place hydro-chloric and nitric acids in different types. And yet, by the present method, they cannot come in the same type, because, first, oxygen cannot come in the hydrogen type, $\left.\begin{matrix} H \\ H \end{matrix}\right\}$; $\left.\begin{matrix} Cl \\ H \end{matrix}\right\}$; and second, in the water-type $\left.\begin{matrix} H \\ H \end{matrix}\right\} O_2$ the oxygen outside of the bracket is differently combined, compared with the oxygen of an oxy-radical replacing H. Thus nitrate of potassa must be $\left.\begin{matrix} N\,O_4 \\ K \end{matrix}\right\} O_2$, and not $\left.\begin{matrix} N\ O_6 \\ K \end{matrix}\right\}$, (since O_4 and O_2 are differently combined,) and chloride of potassium can only be $\left.\begin{matrix} Cl \\ K \end{matrix}\right\}$.

In concluding the subject, it may be observed that by the former method of writing formulæ, the binary nature of chemical compounds, owing to the polarity of their atoms, was kept prominent; while this is not the case with respect to type formulæ, although in these the polarity of the atoms is not denied, but kept in subordinate view.

Whatever be the faults or merits of the type method, it has, by placing bodies before us in a new relation, suggested experiments (which, perhaps, would not have been otherwise suggested) which have led to important discoveries. At the present time, not to understand this method of writing formulæ is to be excluded from following the course of modern chemical progress.

RESEARCHES ON THE PHENOMENA

WHICH CHARACTERIZE AND ACCOMPANY

THE PROPAGATION OF ELECTRICITY

IN HIGHLY RAREFIED ELASTIC FLUIDS.*

BY PROFESSOR A. DE LA RIVE.

Translated for the Smithsonian Institution from the *Memoires de la Société de Physique et d'Historic Naturelle de Geneve*, tome XVII, 1863.

I was led in 1849, in my first memoir on the aurora borealis,† to show that the electric light which is produced in a vacuum of from four to five millimetres is obedient to the action of the magnet. I subsequently found that this experiment, in which, to produce electricity, I at first used an ordinary electric instrument, and then the hydro-electric machine of Armstrong, succeeded still better with Ruhmkorff's induction apparatus. The employment of this apparatus has since supplied the means of studying in a surer and more commodious manner the propagation of electricity in rarefied gases, and thus the assurance has been obtained that, while an absolute vacuum will by no means transmit electricity, the presence in any space of the smallest quantity of ponderable matter in the state of an elastic fluid suffices for such propagation. To the conclusive experiments of M. Gassiot we essentially owe the demonstration of this important principle. It has been observed that the transmission of electricity through elastic fluids is effected with more or less facility, according to the nature and density of the fluid, and that it is accompanied, when the gas is very much rarefied, by an appearance which has been called the stratification of electric light, consisting in the phenomenon of a succession of strata alternately luminous and obscure, presented by the luminous electric discharge. The action of the magnet on this light has likewise been studied. M. Plucker, after numerous and important experiments, has ascertained its law in connecting it with the formation of magnetic curves. Lastly, different explanations have been offered of the stratification of electric light, some based on the peculiar mode of the production of electricity by Ruhmkorff's apparatus, others referring it, not to the character of the apparatus producing the electricity, but rather to that of the medium which propagates it.

The phenomena just cited had awakened in me a lively interest, and I have for some years more particularly studied them. I have encountered great difficulties in this pursuit, as, on account of the necessity, in operating on highly rarefied elastic fluids, of having apparatus which will properly maintain a vacuum, as well as very delicate instruments to appreciate with minute exactness the degree of rarefacation. The establishment at Geneva, conducted by so skilful a machinist as M. Schwerd, has, however, enabled me in a great measure to

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* For a table of French measures, compared with English, see the last page of this Report.

† *Annales de Chimie et de Physique*, tome XXV, p. 310; and *Comptes Rendus de l'Academie des Sciences*, tome XXIX, p. 412.

surmount these difficulties, and to arrive at results which I can with confidence present to the society.

My earlier researches, which had chiefly for their object the study of the general phenomena, were directed only to hydrogen and nitrogen, two gases, differing greatly as regards their physical and chemical properties, and offering, moreover, the advantage of being at once simple, unalterable, and without action on the metals serving as electrodes. Atmospheric air, on which also I have often operated, acts very much as nitrogen, whether because the proportion of oxygen it contains is small in comparison with that of the nitrogen, or because this oxygen, at least in great part, quickly disappears by reason of the transmitted electricity, which, converting it into ozone, facilitates its combination with the metal of the electrodes. I have also, in some cases, mixed with the gas submitted to experiment a little vapor of water or of alcohol.

Electricity has, in my experiments, been produced by a Ruhmkorff induction apparatus of mean force, set in action by one or two pairs of Grove's cups,* and operating by means of the ordinary cut-off. The electricity thus produced is transmitted by means of copper wires covered with gutta-percha through the gaseous mediums, more or less rarefied, contained in glass vessels of different forms, tubes, jars, spherical or ovoid globes, &c. These vessels are to be carefully closed with good taps, and furnished with metallic electrodes of divers forms and natures, which serve to introduce the electric currents.† In the circuit which these currents are destined to traverse we place distilled water in a small glass trough, some twenty centimetres in length by five in width and three in depth. Two plates of platina fixed respectively at the extremities of the trough, and whose surface is exactly equal to the transverse section of the stratum of water, serve to establish this water in the circuit. The purpose of the interposition of the water is to determine the intensity of the electric current by means of an expedient which permits, with that view, the employment of a very delicate galvanometer. Two wires of platina, each inserted in a glass tube, are attached vertically to solid supports, so as to be immersed in distilled water at their lower extremities, which extremities project from the glass only a milli-

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* The battery in question is but a particular form which I have given to Grove's apparatus to render its management more convenient and prompt. It is constructed as follows:

A glass jar with a large opening of about ten centimetres, closed with a glass stopper rubbed with emery, contains about a litre of nitric acid. When the pair is to be used, we remove the stopper and replace it by a porous cylinder of such diameter that it can enter freely into the jar by the opening. This cylinder, long enough to be plunged nearly to the bottom of the jar, has on its upper portion an annular protuberance, by means of which it rests on the edge of the opening. It contains sulphuric acid diluted with water, and a tube or strip of zinc immersed in the acid solution. It is, besides, surrounded externally with a thin plate of platina, to which is soldered with gold a wire, also of platina, which terminates outside, traversing the annular projection of the porous cylinder. The zinc and the platina wire each carry nippers, by which the conductors are readily attached. There may be several similar pairs, and nothing is easier than to arrange them in series, so as to obtain a battery more or less powerful. But a single pair is sufficient, if well mounted, for nearly all electro-dynamic experiments, and particularly for the demonstration of the laws of Ampére, as well as for the production of the phenomena attending the discharge of the Ruhmkorff apparatus in rarefied gases.

It is not necessary often to change the nitric acid, since the jar contains a large quantity. The same acid may serve for several days and for many experiments. It is of advantage, however, frequently to change the acidulated water which fills the porous tube—a very easy and unexpensive operation. Finally, an important precaution to be taken is, that, when we cease to use the pair, the porous cylinder should be withdrawn from the nitric acid, care being taken immediately to replace it with the stopper rubbed with emery, and the cylinder should be immersed in a bottle filled with pure water. Thus the emanations of the nitrous vapors, and the penetration of the nitric acid through the porous cylinder, are avoided. We should guard against immersing the amalgamated zincs in the same water in which the porous cylinders have been plunged, for the smallest trace of nitric acid in water suffices to alter the zincs.

† For electrodes I have chiefly used balls of platina, one centimetre in diameter.

metre, in accordance with the plan of Dr. Wollaston, while their upper extremities communicate respectively with two ends of the wire of a galvanometer, whose coils are well isolated by means of resin. The supports which bear the platina wires are movable along a division in such way that the two extremities of the wires immersed in the water may be made to approach one another as closely as possible, or be separated very nearly the whole length of the stratum of water. By means of a micrometric screw, the relative distance of the two points of platina may be so varied as to be appreciable to nearly the tenth of a millimetre. These two points draw off an almost insensible proportion of the electric current which traverses the trough filled with water—a proportion, however, which suffices to act in a distinct manner on the needle of the galvanometer. The proportion drawn off depends for a current of constant intensity on the distance of the two points, so that, if the intensity be variable, it is the variable distance to which it is necessary that the two points shall be brought, in reference to one another, in order for the indication of the galvanometer to remain constant, which measures the proportion drawn off in each case, and thus, by a ratio easily determined, the absolute intensity of the current.

Finally, a good pneumatic pump, to which a second complementary one may be joined, enables us to bring the gas to an advanced degree of rarefaction. As to the elastic force of the gas, that is measured by a manometer of mercury very carefully constructed, with which, by means of a cathetometer, a difference of pressure of even the fiftieth of a millimetre may be appreciated.

§ I.—GENERAL PHENOMENA PRESENTED BY THE TRANSMISSION OF ELECTRICITY IN RAREFIED GASES.

The Ruhmkorff apparatus, of which I have availed myself, gives in the inducted wire two successive and alternately contrary discharges. Hence, if these discharges encounter in the circuit which they traverse only good conductors, such as metallic wires, and even distilled water, no deviation is remarked in the galvanometer, because the discharges being alternately in a contrary direction, and in rapid succession, their opposed double action is neutralized. But if the circuit comprises an elastic fluid very much rarefied, the resistance which it opposes to the passage of the two successive discharges causes one of them to predominate, so that the phenomena take place as though there were but a series of discharges all in the same direction. The explanation of this difference is, that the two discharges, or inducted currents, though equal in quantity, have not the same tension, the direct, which have a less duration, having a stronger tension. It thence results that when the circuit is interrupted by a body which is a bad conductor, such as an elastic fluid more or less rarefied, the direct currents can alone be transmitted, so that the direction of the inducted current which traverses the elastic fluid is the same with that of the inductive current, and the latter changing, the other changes at the same time.

The pressure at which a discharge of a given intensity begins to pass through a gas varies with the nature of that gas, with its degree of rarefaction, and with the dimensions and form of the vessel which contains it. Moreover, the discharge does not pass immediately upon the electrodes being put in communication with the poles of the Ruhmkorff apparatus. For that a certain time is necessary—a time so much longer in proportion as the resistance is greater, whether arising from the nature or density of the elastic fluid, or from the effect of the form and dimensions of the vessel. Thus, in a long tube, from 2 to 5 centimetres in diameter and from 30 to 50 centimetres in length, it requires several minutes before the discharge can be transmitted, however rarefied the gas. But the first discharge having once passed, the succeeding ones pass with facility, and follow one another so rapidly as to produce on the galva-

nometer the effect of a continuous current. The passage of the discharges may even be interrupted for many minutes without the loss of the capacity which the gas had acquired of immediately transmitting them. To lose it completely we must wait a long time, or renew the gas, and consequently again rarefy it. An equally important fact to be noticed is, that the discharges once transmitted, there may be gradually introduced, while they are passing, a new quantity of the same gas, which amounts to an augmentation of the density, without their ceasing to pass; the pressure may thus be carried to almost double what it was at the beginning. The direction of the discharges has no influence on this train of phenomena; for, the discharge having once effected a passage, its direction may be changed at will, without a cessation of immediate transmission. This result, which I have had occasion to verify in many and very different cases, would seem to show that the gaseous matter opposes a certain inertia to the establishment of that particular disposition which the transmission of electricity requires, and which determines the tension which precedes that transmission; but that this disposition once established, it subsists long after the passage of electricity has ceased, provided no disturbance intervene in the state of the gas. It had long been supposed, particularly in the theory of Grotthus on *electrolytic decompositions*, that something analogous occurred in the transmission of voltaic currents through liquids; it had thence been inferred that the tension of the poles of the piles induced in the liquid in which these poles were plunged, a polarization, which preceded the passages of the current. Only these two effects succeeded one another in a time so short as to be inappreciable, while with gases they would be found to be separated by an interval of more or less duration, but always appreciable.

I shall restrict myself here to certain numerical results,—results of but little importance indeed, since it is impossible to deduce from them a law, in view of the numerous causes which occasion them to vary. They serve only to show the accuracy of the general principle which I have just indicated. We may also infer from them the great superiority of hydrogen over nitrogen and atmospheric air, as regards its conducting power—a fact already noticed by several experimenters.

In a tube 5 centimetres in diameter and 16 in length, the discharge, when the tube was filled with *atmospheric air,* only began to pass when the pressure was reduced to 20 millimetres; with *nitrogen* it passed under a pressure of 24 millimetres, and with *hydrogen* under that of 36 millimetres. It is true that subsequently, under the same conditions of intensity, and still with hydrogen, the discharge passed at pressures of 42, and even 48 millimetres; when rendered still stronger, it has been transmitted under a pressure of even 72 millimetres. With a tube having a like diameter of 5 centimetres, but only one metre in length, the same discharge only began to pass through *nitrogen* under a pressure of from 4 to 5 millimeters; with *hydrogen* it passed only under a pressure of from 12 to 13^{mm}. Afterwards, when stronger, and again with hydrogen, it passed under a pressure of 18, and even 20^{mm}. When the discharge begins to be transmitted, it exhibits itself in very minute jets or streams, more or less intermitted; afterwards these streams combine to form a larger and more continuous one. In a jar filled with hydrogen the discharge passed from an isolated central ball to a ring 12 centimetres in diameter, making the distance of the transmission but 6 centimetres in a space of hydrogen which might be called unlimited. In this case it was transmitted under a pressure of 128^{mm} in the form of streams more or less intermittent and undulating, darting from the central ball to all points of the ring indifferently. At 90^{mm} the discharge gave rise to a continuous stream, susceptible of being influenced by magnetism.

We see, by the instances just cited, that the pressure under which, for any given gas, the discharge can pass varies with so many circumstances, that its

determination is of little importance. Not so, however, when the discharge is once transmitted in regard to the influence which the pressure of the gas it traverses exerts on its intensity. The following are two comparative experiments made with nitrogen and hydrogen. These two gases were successively introduced into a tube 5 centimetres in diameter and 50 in length; the discharge passed between two balls of platina one centimetre in diameter, placed respectively near each extremity of the tube, so that the passage across the gaseous medium was quite 50 centimetres in extent.

Nitrogen.—The intensity of the discharge was measured by means of that of the derived current received by the two points of platina plunged, at a fixed distance of 120^{mm}, in the distilled water which was placed in the circuit:

Pressure.	Intensity of derived current.
9^{mm}	Galvanometer almost insensible.
7^{mm}	$4°$
6^{mm}	$13°$ to $16°$
4^{mm} to 5^{mm}	$26°$ to $30°$
3^{mm} to 4^{mm}	$35°$
3^{mm}	$38°$ to $40°$
2^{mm}	$42°$ to $45°$

Hydrogen.—Proceeding at first as in the case of the nitrogen, the two points of platina which received the derived current were left at a fixed distance from one another:

Pressure.	Intensity of derived currents.
60^{mm} to 30^{mm}	$1°$ to $4°$
26^{mm}	$5°$
18^{mm}	$6°$
15^{mm}	$7°$
13^{mm}	$13°$
10^{mm}	$40°$
9^{mm}	$50°$

For pressure of less than 9 millimetres the points of platina were in each case brought nearer together, so as to have a constant current. At a distance of 55^{mm}, the derived current, which under a pressure of 9^{mm} had been $50°$, was reduced to $40°$. The following series was then obtained; and here, in order to restore the derived current to $40°$, it was requisite, in proportion as the pressure diminished, to bring the points closer together, so as to render the interval of derivation smaller:

Pressure.	Distance of points.
9^{mm}	65^{mm}
8^{mm}	45^{mm}
7^{mm}	30^{mm}
6^{mm}	25^{mm}
5^{mm}	20^{mm}
4^{mm}	17^{mm}
3^{mm}	14^{mm}
2^{mm}	12^{mm}

Thus, as far as 2^{mm} of pressure, the intensity of the derived current, and consequently the conductibility of the gas, goes on increasing as well for the hydrogen as for the nitrogen; but we see how much more considerable is the conducting power of the hydrogen than that of the nitrogen, since, under a pressure of 9^{mm}, all other circumstances remaining the same, the derived current is, with the nitrogen, scarcely sensible, while it is $50°$ with the hydrogen.

In two other comparative experiments, the pressure and the distance of the electrodes were made to vary alike for the nitrogen and hydrogen. The two gases had been successively introduced into the same ovoid globe. The following table gives the intensity of the derived current for three different distances of the electrodes under different pressures, when *nitrogen* is in the ball:

Pressure.	Distance of the electrodes.		
	14 cent.	7 cent.	1 cent.
20^{mm}	10°	23°	55°
8^{mm}	40°	47°	55°
5^{mm}	50°	55°	57°
3^{mm}	55°	55°	57°

When for nitrogen we substitute *hydrogen*, the results differ somewhat, especially in the lower pressures, and when the electrodes are in close proximity with one another, which proceeds probably from the circumstance that the gaseous medium, in view of the form of the vessel, may be regarded as having an almost indefinite breadth. In a large receiver, in effect, where the current passes between a central ball and a concentric ring having a diameter of 12 centimetres, the intensity of the derived current is very little increased by diminishing the pressure beyond 10^{mm}. That intensity, measured by the derived current, amounts, under a pressure of 15^{mm}, to 35°; under a pressure of 10^{mm}, it attains 45°; then augments gradually as far as 5^{mm}, when it reaches its maximum of 50°, which it never exceeds, manifesting rather a slight tendency to become less under 2^{mm}. By raising the central ball so as to give to the electric sheet a conical instead of circular form, the conductibility is not sensibly diminished. Under the same circumstances the atmospheric air does not present a resistance much greater than the hydrogen; thus the intensity of the derived current is 35° at 5^{mm} instead of 50°, and at 2^{mm} is 45°. However, with the tube of one metre in length, hydrogen must be subjected to a much weaker pressure in order to transmit the discharge, but its conductibility increases very rapidly with the diminution of that pressure. Thus, the apparatus of derivation being placed in the circuit, we have:

Pressure.	Intensity of the derived current.
5^{mm}	0°
4^{mm}	12°
3^{mm}	22°
3^{mm}	30°
2^{mm}	52°

Finally, with the same tube, one metre in length and 5 centimetres in diameter, a sensible and regular augmentation in the intensity of the derived current has been obtained, for the same pressure and in the two gases alike, by a corresponding diminution of distance between the electrodes. The comparison of the numbers indicates, that, when the gas is sufficiently rarefied to be a good conductor, that is, to permit the discharge to become, so to say, continuous, it follows, like liquids and solids, in its conductibility, the law of the inverse of the length. It has been already seen that the influence of the section and of the volume is very considerable; but I have not been able to determine its law in a precise manner.

Thus far I have considered the propagation of electricity in gaseous substances only in relation to the resistance they oppose to it—a resistance variable with their nature, their density, and their dimensions. I have but glanced at this part of my subject, to which I shall return at an early occasion, as I propose to extend my researches to a much larger number of gaseous substan-

ces. I pass now to phenomena of quite another order, and which relate to the mode itself in which the propagation of electricity is effected in gases—a mode which manifests itself under the form of stratification of the electric light.

§ II.—INVESTIGATIONS REGARDING THE STRATIFICATION OF THE ELECTRIC LIGHT.

It is known that at a certain degree of diminution of the elastic force of a gas which transmits the electric current, that current becomes stratified—that is to say, is decomposed into strata alternately obscure and luminous. The stratification commences by the appearance of certain slight striæ or furrows on the side of the positive electrode; then gradually, as the elastic force diminishes, the current, which was at first very narrow, dilates, and the striæ grow larger. Next appears an obscure space, separating the extremity of the luminous column from the negative electrode, which is itself surrounded with a bluish atmosphere. This atmosphere continues to dilate, and the obscure space to lengthen, in proportion as the rarefaction of the gas increases.

In order to obtain the stratification of the electric light, it is necessary to diminish the pressure of a gas in proportion as the gas offers more resistance to the transmission of electricity. Thus in hydrogen, under a pressure of 18^{mm}, the electric stream, which consists as yet of but a small rose-colored filament from three to four millimetres in diameter, is seen to divide into very distinct circular sheets, alternately obscure and luminous, the breadth of which is one-fourth of a millimetre. These striæ, at first more distinctly marked at the positive electrode, become general throughout the whole electric current, whatever be its length; and, in proportion as the pressure diminishes, the stream becomes enlarged, so as even to occupy the whole interior of a tube five centimetres in diameter. At the same time the breadth of the alternately obscure and luminous divisions so increases that, under a pressure of 2^{mm}, it is about 5^{mm}. These divisions are themselves annular, as I have satisfied myself by closing the tube which contains the rarefied gas, at one of its extremities, with a glass disk, which permits the whole interior of the tube to be seen in the direction of its length.

When the striæ begin to appear, an obscure space, as has been said, is seen to form in front of the negative electrode, increasing in proportion as the pressure diminishes, so far as finally to occupy a length of ten centimetres—a length which is independent of that of the gaseous column. However, by observing with attention this obscure space, we discover, beyond an interval which is perfectly black, and of a well-defined length of from 2 to 3^{mm}, a palish, rose-colored gleam, which is only visible in utter darkness. This gleam, which has the form of a cone, whose base is the last section of the luminous column, only appears when the pressure has become very slight and quite inferior to that under which the obscure space is manifested. It is accompanied by the appearance, in the same obscure space, and at unequal intervals, of several still more luminous rings, (I have counted as many as four,) which contrast, by their immobility and their well-defined outlines, with the agitated striæ or divisions of the rest of the current. Let us add, that the luminous and stratified part of the current, which is much the longest, is so much the more distinctly and sharply separated from the obscure or palish part, as the electric discharge is more intense.

The bluish atmosphere which surrounds the negative electrode is also enlarged in proportion as the pressure diminishes, and nearly in the same ratio as the striæ. At the same time, its brightness becomes less vivid, and its exterior outline less sharply defined. This bluish atmosphere, which at first enveloped only the negative ball, at last, and in proportion as the pressure diminishes, equally envelops, in all its length, the metallic rod which supports the ball; at least, if this be not covered with an isolating coat, which indicates,

on the part of negative electricity, a great facility in dispersing itself in the ambient medium, when once that medium is rarefied.

The agitation of the striæ in the luminous part of the current becomes very considerable under the slight pressure of two millimetres. It manifests itself at first very sensibly in the neighborhood of the positive electrode, from which the luminous stream issues under the form of an outspreading cone, which, in proportion as the pressure diminishes, becomes more and more cylindrical, until it assumes altogether the form of a cylinder of whose circular base the electrode is the centre, the agitation of the striæ being, at the same time, general throughout the whole extent of the current.

When the discharge is effected in a cylindrical jar, between a ball serving as a negative electrode and a metallic ring of which that ball is the centre, and which serves as a positive electrode, the bluish atmosphere which surrounds the ball enlarges by several centimetres at a pressure of 2^{mm}, and its exterior outline is covered with small filaments, presenting a tuftlike appearance. These filaments are probably formed by the series of molecules which transmit the discharge. They are much more distinct with hydrogen (a good conductor) than with other gases. If the ball serves as a positive electrode, it is surrounded with a lively rose-colored halo of about a centimetre in diameter, presenting well-marked stratifications; then comes a dark annular space, which terminates at the ring, which is itself completely invested with an envelope or sheath of clear violet, with opaline tints.

Nitrogen presents the same phenomena as hydrogen, though the stratification of the electric light does not begin, except under a much feebler pressure. In the long tube (one metre in length) the agitation of the striæ, under a pressure of 2^{mm}, is even more considerable than with hydrogen. These striæ seem to form an animated helix, with a movement of rotation around its axis. The light is also more vivid, the tint being of a peach-blossom rather than pale rose, color. The phenomenon is of a most brilliant description. Further, there is the same obscure space in the vicinity of the negative electrode, the same glimmer of palish rose color at a weak pressure of from 1 to 2^{mm} in this obscure space, the same appearance in this glimmering mist of well-defined and motionless rings more luminous than the space which surrounds them.

Atmospheric air corresponds in its phenomena with nitrogen. I have observed only that here the agitation of the striæ is less striking, and the light of a rose color less deep than in nitrogen.

The appearances which I have just described are, therefore, within some mere shadings, precisely the same in hydrogen, nitrogen, and atmospheric air; they are equally the same, whether these gases are dry, or contain the vapor of water or of alcohol in more or less quantity; there are no differences, except that the pressures at which the various phenomena, and the tints of light which accompany them, are observed, vary with the nature of the rarefied elastic fluid. We cannot, then, attribute the effects just considered to an electro-chemical decomposition which cannot take place in a simple and well-desiccated gas, nor to any action appertaining to the chemical nature of the elastic fluid. They are evidently the result of a mechanical action which accompanies the transmission of electricity—an idea first advanced by M. Riess, who showed that an analogous phenomenon presents itself, under a little different form, it is true, in liquids and in solids.

The phenomenon in rarefied elastic fluids would consist in the alternate contractions and dilatations of the gaseous medium produced by the series of discharges, always more or less intermittent, of which the electric stream is formed. In fact, whether it be by Ruhmkorff's apparatus, or an ordinary electric machine, or by a hydro-electric machine of Armstrong, that the stratifications are produced, there is never a continuous discharge, but, in reality, a series of discharges which may succeed one another so rapidly that the intermission shall

not be betrayed, even by a galvanometer. But it does not the less exist, as M. Gassiot has shown in operating with a pile of Grove at high tension, which, with the same electrodes, and in the same medium, will give rise, first to stratifications, and afterwards, when the current has become continuous, to a voltaic arch.

The mechanical action of the series of discharges on the rarefied elastic fluid may, indeed, be directly verified by the very marked oscillations of the column of mercury of the manometer placed in communication with the elastic fluid, which accompany the propagation of electricity in that fluid. These oscillations rise to two or three tenths of a millimetre in hydrogen, under a pressure of 16^{mm}. They begin to be sensible when once the stream passes, that is to say, at 36^{mm} of pressure; attain their maximum of three-tenths of a millimetre between 20 and 12^{mm} of pressure; and diminish rapidly in descending from 12 to 5^{mm}, at which last pressure they no longer take place. With nitrogen and atmospheric air, and employing the same tube 16 centimetres in length and 5 in diameter, the oscillations begin, at the moment when the stream passes, under the pressure of about 20^{mm}; attain their maximum of from four to five tenths of a millimetre between 12 and 8^{mm} of pressure; and then continue to diminish until 2 or 5^{mm}, at which pressure they cease to be sensible.

With the tube one metre in length, and even with that of 50 centimetres, I have not succeeded in observing any appearance of oscillation accompanying the transmission of the electric current, whatever might be the gas enclosed in these tubes, and whatever the pressure to which it was subjected. On the other hand, I have obtained very distinct ones, of one and two tenths of a millimetre, under pressures varying from 30 to 15^{mm}, in a jar 20 centimetres in height by 16 in diameter, filled with rarefied hydrogen, and in which the electric stream passed from a central ball to a ring 12 centimetres in diameter concentric to that ball. This last result shows that the absence of oscillations in the long tubes has less connexion with the volume of the gaseous medium, which is less than in the vessel of the last experiment than with the influence of the sides of the tubes which embarrass the movement of the gas. It is also a proof that the oscillations proceed from a mechanical action, and not from an elevation of temperature. As regards their intensity, the oscillations evidently depend on the greater or less resistance which the gaseous medium opposes to the transmission of the electric current, since the oscillations are more considerable with nitrogen than with hydrogen, and diminish as the pressure does, reckoning from a certain point of the pressure, which is that at which the discharge can take place in a complete manner, and at which the intensity of the oscillations attains its maximum.

The stratification of electric light would appear then to be a phenomenon analogous to the production of undulations of sound, that is to say, a mechanical phenomenon proceeding from a succession of isochronous impulses communicated to the rarefied gaseous column by the series of electric discharges rapidly succeeding each other. We find a new proof favorable to this view of the phenomenon in the perturbation which a displacement of the gaseous matter occasions in the stratifications, and, consequently, in the disposition of the elastic fluid which permits those stratifications to appear. To produce this perturbation, it suffices to introduce into the tube in which there is a rarefied elastic fluid, and while the electricity is in process of propagation, an additional quantity of the same gas already enclosed therein, so as to increase the pressure by one-fourth or one-half of a millimetre at most. Let us see what then occurs with hydrogen, remarking that the effects are the same with the three tubes, respectively, 15, 50, and 100 centimetres in length.

We begin by rarefying the gas to the extent of 2^{mm}, so as to have the phenomenon of the stratifications as distinct as possible. We then introduce a small quantity of hydrogen; if the introduction takes place on the side of the

negative electrode, striæ of a fine rose color are immediately seen to form in the obscure space, their diameter being that of the stratified column—that is, of the tube, while they are at the same time very narrow and well defined. They are gradually propagated in the tube, confounding themselves with the original striæ, which are much larger and less distinctly limited; then, as soon as the entrance of the gas is arrested, the luminous column is seen to recede slowly from the negative electrode, and resume gradually its primitive appearance. When the introduction of the gas takes place on the side of the positive electrode, in place of the striæ occupying the whole cavity of the tube, we see a brilliant stream of very small diameter (2 to 3^{mm}) distinctly striated, and quite similar to a minute spiral spring *(ressort a boudin,)* advance along the axis of the tube in the relatively obscure interior of the luminous column, which itself, as soon as the gas begins to enter by the positive as well as by the negative electrode, immediately advances so as to occupy almost entirely the obscure space up to the negative electrode, from which it is only separated by the interposed stratum, 2^{mm} in thickness, which it cannot surmount. Then, the introduction of the gas once stopped, everything returns quickly to the normal state. By whichever of the two extremities of the tube the gas is made to penetrate, we see, on the entrance of the gas, a very subtle mist of a roseate white color make its appearance, and diffuse itself in the tube; but this, as soon as the introduction of the additional quantity of gas has ceased, passes over from the negative to the positive electrode, leaving the obscure space to form itself anew, and momentarily hiding in its passage, by enveloping them as it were with a light cloud, the successive stratifications of different parts of the column; then this mist disappears, and the luminous column resumes its primitive appearance. which it maintains so long as nothing is changed either in the electric current or the state of the gas traversed by it. The appearance of this mist, which perfectly resembles that I have mentioned as existing in the dark space of the column in a state of repose, well denotes the agitation into which the introduction of a small additional quantity of gas throws the whole column—an agitation so conspicuously manifested by the progression of the striæ and their encroachment on one another. The phenomenon presents this further feature: that the definiteness and brightness of the striæ in the gaseous portion introduced, which make them so plainly distinguishable from the gas which was already in the tube, enable us to follow the progressive movement of that portion from one end of the tube to the other. The experiment may be repeated several times in succession by successive introductions of additional quantities of gas, provided that each time the pressure be not increased more than $\frac{1}{4}$ of a millimetre, and that the total pressure do not in all exceed 5 or 6^{mm}.

With nitrogen and atmospheric air the incidents are the same, only we cannot push the experiment so far, the pressure at which the phenomenon ceases to take place with these gases being much less than it is with hydrogen. The narrow striæ which display themselves at the moment of the entrance of the gas on that side where the entrance takes place are also less distinct and less brilliant, but there is equally a momentary disappearance of the obscure space, the production of a roseate mist, and progression of this mist, on the cessation of the introduction of gas, from the negative electrode to the positive. With the three gases alike, we see, when the introduction is effected on the side of the negative electrode, the mist advance at first like the slender striated thread which follows the axis of the tube from the positive electrode to the negative; then, having arrived at this extremity of the tube, it turns back, passing over, as has been said, from the negative to the positive electrode.

This mist evidently proceeds from a portion of the gas which, in entering the tube, is excessively dilated, and becomes visible by the electricity which traverses it. From the slowness with which the mist is propagated we may judge of the feeble degree of elastic force in the gas. It is to the same cause probably

that we should ascribe the slowness with which the mixture of the gas which is entering the tube with that already present is effected—a slowness which is manifested by the circumstance of the definite and narrow striæ appearing in the new portion of gas, while in the old the striæ are much larger, and by no means so distinctly defined—a phenomenon which can only proceed from the former not being, at the moment when it enters the tube, so much dilated as the gas which was already there. In fine, the fact that the gaseous column with narrow striæ is much larger when the gas which produces it enters on the side of the negative electrode than when it enters on that of the positive, is a proof that, before the new introduction of gas, the gaseous column already in the tube was much more dilated in the neighborhood of the negative electrode than on the side of the positive. So, then, the passage of the electric discharges very rapidly succeeding one another across a rarefied gaseous column would produce therein, when the rarefaction had reached a certain degree variable with the nature, and consequently with the conductibility of the gas, first, a considerable dilatation of the gaseous matter around the negative electrode, and next, beginning in this dilated portion of the column, a succession of alternate contractions and dilatations as far as the positive electrode. It is highly probable that the same effect takes place when the gas is not sufficiently rarefied for producing stratification of the electric light. But in that case, the greater elastic force of the gas, joined with the necessarily less rapid succession of the discharges, allows the immediate return of the contracted and dilated strata to their state of normal density, and thus prevents that double state from manifesting itself; while when the gas is less elastic, and the discharges succeed one another more rapidly, the state of dilatation and contraction of successive strata produced by a first discharge still subsists when a second arrives, the result being that it becomes sensible.

The transmission of electricity, then, through a gaseous column occasions a movement in the particles of gas, and that movement seems to be an impulse emanating from the negative electrode. Might not this effect be attributed to the static electricity with which the molecules are charged, and which would augment their constitutive repulsion? We know, and it is seen by the luminous aureoles which surround the negative ball and rod, that, at an equal tension, the negative electricity issues more readily than the positive from its metallic electrodes in order to penetrate into the rarefied ambient medium. Hence, the portion of that medium nearest to the negative electrode must be more charged with static (negative) electricity than is (with positive) the portion of the rarefied gas near the positive electrode; it is not, then, surprising that the repulsion of the gaseous molecules, and consequently the rarefaction of the gas, should be greater in the first of these two portions than in the second.* Now, why does negative electricity diffuse itself more easily than positive under the same conditions of intensity, magnitude, and position of electrodes, nature and rarefaction of the ambient medium? Here is the mystery, or at least a point of most interesting consideration as regards the theory of electricity.

§ III.—PARTICULAR PHENOMENA PRESENTED BY DIFFERENT PARTS OF THE STRATIFIED ELECTRIC CURRENT.

The gaseous column traversed by the electric current is composed, as we have said, when it has been brought to a certain degree of rarefaction, of strata alternately dilated and contracted, with an obscure space greatly dilated in the neighborhood of the negative electrode. The more dilated parts of the column offering less resistance to the transmission of electricity must remain obscure.

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* The fact that the electricity of tension is more easily propagated around the negative than around the positive electrode may be readily verified by experiment, as well as the permanent state of electric tension of the gaseous column during the passage of the electric current, whatever may be the rarefaction of the gas.

while the more contracted, with less capacity of conduction, grow warm, and become luminous, even when it is the same discharge which traverses them. We should here expect a phenomenon exactly analogous to that which is produced when we place in the circuit of a voltaic pile a chain formed of alternate wires of platina and silver, having the same length and diameter; although they both transmit the same current, the wires of platina, offering most resistance, grow hot, and become even incandescent, while those of silver, being better conductors, remain cold and opaque.

To demonstrate that in fact the space remaining opaque offers less resistance to the transmission of electricity in the stratified column than the luminous part of that column, I have arranged two small disks of platina, 7^{mm} in diameter, each attached by a point in its circumference to the end of a wire of platina, enclosed in a tube of glass, in such a way as to be kept parallel to one another at a distance of three centimetres. The two disks are connected in a solid manner, though very carefully isolated, and without any possible electric communication except by means of the wires of platina soldered to their circumference, and enclosed in a tube of glass. The free extremities of the two wires of platina can be respectively placed in communication with those of the wire of a galvanometer. The apparatus is arranged in such manner that the two disks of platina may be introduced into the stratified electric stream so as to cut it transversely, and to have their centres situated in the very axis of the stream. They thus serve as *sounds* destined for the derivation of a part of the current, and the intensity of that derived portion, which is so much less as the conductibility of the interval of derivation is greater, is measured by the deviation of the needle of the galvanometer put in communication with the free extremities of the platina wires which support the disks; these wires, as has been said, are themselves enclosed in tubes of glass where they traverse the recipient which contains the rarefied gas, with a view to their remaining well isolated, and that the disks alone may be in contact with the gaseous substance which transmits the discharges. Now, it suffices to change the direction of these discharges in order that the sounds, without being displaced, shall be immersed either in the obscure space near the negative electrode, or in the luminous space near the positive one. The apparatus is, moreover, so contrived that the sounds may be placed in other portions of the current. It is proper to add, that the electrodes between which the electric stream passes are two disks of platina, each five centimetres in diameter, placed parallel to one another at a distance which may vary from forty to thirty centimetres, and consequently, like the little disks serving as sounds, perpendicular to the axis of the stream.

The following are some experiments made successively with nitrogen and hydrogen:

NITROGEN, OR ATMOSPHERIC AIR.

Pressure of the gas.	Intensity of the derived current.	
	Sounds near the positive electrode.	*Sounds near the negative electrode.*
6^{mm}	70°	18°
4^{mm}	40°	8°
2^{mm}	18°	3°

HYDROGEN.

15^{mm}	90°	90°
6^{mm}	82°	65°
4^{mm}	52°	2°
2^{mm}	35°	0°

We see from these tables that the intensity of the derived current diminishes with the pressure, although the transmitted current be much stronger, which shows with what rapidity the resistance of the gas diminishes in proportion as its rarefaction increases. But at the same time the diminution of the derived

current, and consequently that of the resistance, is much greater when the sounds are immersed in the obscure space near the negative electrode than in the luminous part of the stream near the positive one. Thus, under the pressure of 2^{mm}, it is impossible in hydrogen to perceive the least derived current in the black space, while this derived current is at the same time 35° in the luminous space; at a pressure of 15^{mm} it was 90° in the neighborhood of the two electrodes alike, but there was as yet no formation of the obscure space, and consequently the state of density of the gas was the same at the two extremities of the tube. The resistance of the obscure space is also very feeble in nitrogen under a pressure of 2^{mm}, since the derived current is only 3°, while it is 18° in the luminous space; but the difference between the two derived currents is less than in hydrogen. This difference results from the fact that hydrogen, having a conductibility much superior to that of nitrogen on the one hand, the absolute intensity of the current is greater, which explains why we have 35° instead of 18° in the luminous space; on the other hand, the derived portion must then be less where rarefaction still more augments the conductibility of the gas, which accounts for our having 0° in place of 3° in the obscure space.

Let us here remark, that all the results which show the unequal resistance presented by different parts of the same gaseous column to the propagation of electricity are readily comparable with one another, since it is the same electric stream which successively traverses these different and unequally conducting parts.

If we place the sounds in a portion of the stream which is $\frac{1}{3}$ of the distance from one of the electrodes, and consequently $\frac{2}{3}$ from the other, we have for the intensity of the derived current, under a pressure of 2^{mm} in *air* or *nitrogen*, 8° when the negative electrode, 12° when the positive, is nearest to the sounds. In hydrogen we have 20° and 36°. Thus, the conductibility of the gaseous column goes on diminishing gradually from the obscure space, where it is at its maximum, to the space near the positive electrode, where it is at its minimum.

By placing the sounds always in the same portion of the stream, we can find in the intensity of the derived current a sufficiently exact expression of the degree of resistance of different gases at different degrees of pressure, provided we take care, by means of a rheostat, to give to the principal current in each case the same degree of absolute force. This is an investigation with which I am at present occupied, and which is not yet finished.

We see, then, that the obscure space near the negative electrode offers much less resistance to the passage of the current than does the luminous part near the positive electrode. It thence results that, for the same reason that the less conductive portion of the gaseous column is more luminous than that with greater conducting capacity, which remains nearly dark, the temperature of the first should be higher than that of the second—an inference which experiment has fully confirmed.

Two thermometers of mercury, with cylindrical reservoirs, were placed in the interior of the tube, which is 16 centimetres in length and 4 in diameter, at the respective distance of one centimetre from each of the electrodes—a distance sufficient, as was ascertained, to annul the cooling or heating influence of those electrodes. That the influence would rather have been refrigerant, was found susceptible of verification by bringing them nearer the reservoir of the thermometers, which is not surprising, in view of their dimensions, (full metallic balls one centimetre in diameter.)

By causing the electric stream to traverse the rarefied nitrogen or hydrogen, a great difference was at once perceptible between the temperature acquired by the thermometer placed in the dark space near the negative electrode and that acquired by the thermometer placed in the luminous space near the positive electrode. These differences observe nearly the same ratio between the press-

ures of 1 to 10^{mm}, even when the absolute temperatures, with which they must not be confounded, vary with the pressure and with the nature of the gases. Thus, even when there is no longer any sensible obscure space at the negative electrode, the thermometer is less elevated there than in the neighborhood of the positive, which proves that the gas is there more dilated and of more conducting capacity. The difference of temperature, then, should be a still more sensible criterion than the difference of brightness, of the greater or less electric resistance of different parts of the gaseous column. The absolute temperature is in general less in hydrogen at all degrees of rarefaction than in nitrogen and atmospheric air, which offer more resistance to the passage of electricity. The difference between the two thermometers was, moreover, never so great in hydrogen as in nitrogen, or atmospheric air. Thus it was at the maximum of 4½°* in *hydrogen*, under the pressure of 9 to 12^{mm}, the thermometer having risen, in *two minutes*, from 21° to 26½° at the negative electrode, and 21° to 31° at the positive. In *nitrogen* the maximum difference was 5° under the pressure of 5^{mm}, (20° to 24° at the negative thermometer, 20° to 29° at the positive.) In *atmospheric air* the maximum difference was, at a pressure of 6^{mm}, 6°, (from 18° to 26° at the negative thermometer, and from 18° to 32° at the positive.) At a pressure of 20^{mm} the difference was not more in *hydrogen* than 2½°, (from 21° to 28½°, and from 21° to 26°;) in *nitrogen* but a half degree, (from 20° to to 25°, and from 25° to 25½°;) and in *atmospheric air* it was null, (from 19° to 28° at the two thermometers alike.) When there is no longer a difference between the indications of the two thermometers, or that difference is very slight, it will be observed that the appearance of the luminous stream is perfectly uniform through its whole extent.

Here we give a table of some experiments:

ATMOSPHERIC AIR, (*duration of the experiment, two minutes.*)

Pressure.	Positive thermometer.	Negative thermometer.	Difference.
2^{mm}	16° to 25°	16° to 21°	4°
4^{mm}	18° to 31°	18° to 25½°	5½°
6^{mm}	18° to 32°	18° to 26°	6°
8^{mm}	18° to 31°	18° to 27½°	3½°
10^{mm}	18° to 31°	18° to 28°	3°
15^{mm}	18½° to 31°	18½° to 29°	2°
20^{mm}	19° to 28°	19° to 28°	0°

NITROGEN, (*duration of the experiment, two minutes.*)

Pressure.	Positive thermometer.	Negative thermometer.	Difference.
2^{mm}	19° to 24°	19° to 22°	2°
4^{mm}	20½° to 28°	20½° to 25°	3°
5^{mm}	20° to 29°	20° to 24°	5°
6^{mm}	20° to 31½°	20° to 27°	4½°
9^{mm}	20° to 31°	20° to 27°	4°
15^{mm}	21° to 30°	21° to 27°	3°
20^{mm}	20° to 25½°	20° to 25°	½°

HYDROGEN, (*duration of the experiment, two minutes.*)

Pressure.	Positive thermometer.	Negative thermometer.	Difference.
2^{mm}	21° to 27°	21° to 25°	2°
5^{mm}	20° to 28½°	20° to 25½°	3°
6^{mm}	21° to 29°	20° to 25½°	3½°
9^{mm}	21° to 31°	20° to 26½°	4½°
15^{mm}	21° to 30°	21° to 26°	4°
20^{mm}	21° to 28½°	21° to 26°	2½°
30^{mm}	21° to 25°	21° to 23½°	1½°

* The thermometric indications are in degrees of Reaumur.

The following is the result of an experiment in which the duration of the passage of the current was prolonged beyond 2 minutes, through atmospheric air at a pressure of 5^{mm}:

Duration of passage.	Positive thermometer.	Negative thermometer.	Difference.
2′	18° to 31°	18° to 26°	5°
4′	— to 37°	— to 30½°	6½°
6′	— to 40°	— to 32°	8°
8′	— to 42°	— to 33°	9°
10′	— to 43°	— to 34°	9°

In proportion as the duration of the experiment increases and the absolute temperature rises, the differences between the temperatures indicated by each of the two thermometers become proportionally less; the indications of the two thermometers end by approximating, and even becoming the same after the lapse of a certain time. Hydrogen and nitrogen give the same results.

The numbers which express the temperatures in the preceding tables cannot be given as being of perfect exactness; they vary, in effect, in their absolute values with the intensity of the electric stream; but they are sufficiently constant and exact to demonstrate: 1st, that there is a sensible elevation of temperature, which accompanies the propagation of the electric current in rarefied gases; 2d, that this elevation is sensibly less in the neighborhood of the negative electrode than near the positive, when once the gases are sufficiently rarefied for the discharge to pass easily and the electric light to be stratified; 3d, that the absolute elevations of temperature at the two electrodes, and their differences, vary with the density and the nature of the gas.

A fact which shows well all the calorific and luminous power of electricity, is, that hydrogen reduced to $1\frac{1}{2}^{mm}$ of pressure can become luminous and be heated in a very sensible degree* by the passage of electricity, although at that pressure it has a density so inconsiderable that a cubic centimetre of the gas does not weigh more than barely $\frac{1}{5000}$ of a milligramme.

When we see matter so subtle as hydrogen reduced to one or two millimetres of pressure, becoming luminous under the influence of electricity, the temptation can hardly be resisted of surmising an analogy with the matter at once so subtle and so luminous which constitutes the cometary bodies. This analogy becomes still more striking when we examine closely the appearance presented, in the tube which contains the rarefied hydrogen traversed by the electric current, by those species of luminous mists which manifest themselves at the moment when we introduce a little gas into the tube, and which we also see in the obscure space when a certain degree of rarefaction has been attained. Undoubtedly the gaseous matter is there still more rarefied than it is in the rest of the mass where it is already extremely so, and it offers at the same time a remarkable resemblance with the luminous matter which constitutes the comets.

§ IV.—INFLUENCE OF MAGNETISM ON ELECTRIC CURRENTS PROPAGATED IN HIGHLY RAREFIED GASEOUS MEDIUMS.

This influence, whose existence I have shown under the form of a rotation communicated by the pole of a magnet to the electric currents which radiate from it, is, as might be expected, and, as M. Plucker has evinced by several remarkable experiments, general. The luminous filaments which display themselves in rarefied gases, traversed by the discharges of the Ruhmkorff apparatus,

* The heating of the gas must in fact be very considerable to be capable, in two minutes, of raising by nearly 3° the temperature of a thermometer whose reservoir is a cylinder of mercury 2½ millimetres in diameter by 3 centimetres in length. Besides, the single fact that the gas is luminous well evinces its high temperature; for the light is evidently but the effect of its incandescence.

are attracted or repelled by magnets as electric currents circulating in metallic wires would be. In a word, this action is subject to all the laws of electro-dynamics, with this difference, that all the parts of the mobile conductor being independent of one another, instead of being united with one another, as they are in a rigid wire, they completely obey the forces which act upon them, and take the positions of equilibrium which result therefrom. Hence it is that the luminous filament takes the form of a magnetic curve; a necessary condition, in order that the equilibrium should take place, since the action of the magnet on the element of the current is then nothing, the direction of the action being perpendicular to that element when it is a tangent to the magnetic curve.

I have verified in sundry cases the law just recited, and have even succeeded in showing that, conformably to the law of Ampère, two electric streams having the same direction in a rarefied gas attract each other as two voltaic currents transmitted across movable metallic wires would do. I have not realized the repulsion of two electric streams passing in contrary directions, by reason of the practical difficulty which I have hitherto encountered in constructing an apparatus for the purpose. I do not, however, renounce the hope of being able to do so. I shall return to this subject in an article in which I propose to consider the mutual action of electric currents on one another. I restrict myself, for the present, to an investigation of the effects of magnetic action on those currents.

My researches on this subject comprise two series of experiments: first, those in which the electro-magnet from which the electric action emanates is placed externally to the rarefied gas through which the electric stream is propagated; secondly, those in which the magnetized iron is situated in the gas itself.

One of the most simple cases is that in which one of the tubes of which I have spoken in preceding experiments is placed either axially or equatorially in relation to the poles of a strong electro-magnet. The following is what is observed when care has been taken to rarefy well the gas which transmits the electric current. The portion of this current submitted to magnetic action is condensed towards the walls of the tube in the part nearest, or that most remote from, the magnetic poles, according to the direction of the current and that of the magnetization; the striæ become much more compressed and more brilliant. If the portion of the tube placed in the neighborhood of the electro-magnet is that where the negative electrode happens to be, the obscure space is immediately seen to become luminous, and to present close and brilliant striæ as would be the case with the constantly luminous portion of the current which seems to advance. At the same time, the bluish photosphere which surrounds the negative ball contracts to at least half its size, becoming more brilliant, and the sort of bluish sheath which surrounded the metallic rod, at the extremity of which is the negative electrode, completely disappears. All that bluish atmosphere is concentrated on the ball. It seems that all the gaseous filaments, which may be considered as so many conductors of the discharge, instead of radiating from all points of the negative ball and rod, and being disseminated through the entire gaseous mass as far as the positive electrode, radiate only, when the magnetic action is exerted on them, from the negative ball, becoming condensed towards the walls of the tube, on one side or the other, as far as that portion of their course at which, the action being no longer sensible, they resume their normal position. This condensation explains why the part of the current which was obscure because the gas was there too much dilated, becomes luminous, and why that part which was already luminous becomes more slender and brilliant, with stratifications more closely compressed. The action of the magnet produces the same effect which would be produced by a local augmentation of density in the rarefied gaseous matter. Further, it is not necessary that the action of the magnet should take place exactly on the obscure part in order to its becoming luminous; it equally becomes so, even when the magnetism acts

on another portion of the current, provided it be not too remote from the negative electrode.

The consequence of the explanation just given, and which it is easy to verify by experiment, is, that the portion of the gas which transmits the discharge must, when it is subjected to the action of the magnet, become a more imperfect conductor, and that consequently the electric current must, on the whole, encounter a greater resistance in its passage along the interior of the tube when one part of the tube is approached by the electro-magnet than it encountered previously.

Thus the tube of one metre being filled with rarefied hydrogen, and the apparatus of derivation placed in the circuit,* we obtain the following results:

Pressure.	Intensity of the derived current.		
	Without magnetization.	*Magnetization at the positive electrode.*	*Magnetization at the negative electrode.*
4^{mm}	33°	30°	20°
8^{mm}	30°	30°	10°

With the tube 50 centimetres in length, filled with nitrogen, we have:

Pressure.	Intensity of the derived current.		
	Without magnetization.	*Magnetization at the positive electrode.*	*Magnetization at the negative electrode.*
2^{mm}	57°	52°	42°
4^{mm}	37°	27°	17°
6^{mm}	25°	20°	12°

The effects are more marked when the tubes are placed equatorially between two soft-iron armatures of the electro-magnet, which are immediately in contact with the walls of the tube, than when they are placed axially on the poles themselves. We see that there is a much greater increase of resistance when the magnetism acts on the portion of the current near the negative electrode than when it acts on the portion near the positive electrode. The reason of this difference is, that the first portion which, as we have seen in the preceding paragraph, is endued with a much greater share of conductibility, must naturally experience a more considerable diminution of that property by the condensation of the gaseous matter produced by the action of the magnet, than is experienced by the second portion, where the gas is less rarefied. The direction of the magnetization has no influence on the results: it has no other effect than to elevate or depress the current, which, when the magnet does not act, is simply horizontal.

Among the experiments which I have made regarding the influence exerted by the exterior action of magnetism on rarefied gases enclosed in tubes, I will further cite those in which the tube is convoluted into a flat spiral terminated by two prolongations perpendicular to the plane of the spiral which serve to introduce and rarefy the gas, as well as to give a passage to the discharges; the tube of the spiral and its prolongations is a little less than one centimetre in diameter, and its total development nearly eighty centimetres. It is necessary that the gas should be rarefied at least as much as 2^{mm} for the discharges to pass when nitrogen or atmospheric air is employed. With hydrogen, a pressure of 5 or 6^{mm} suffices for their transmission. But whatever the gas or its degree of rarefaction, it is only after the lapse of some minutes from its being placed in the circuit that the discharge begins to pass. It is evidently necessary that it should be sometimes charged with static electricity for the resistance to the establishment of the continuous stream to be surmounted. But that

* It should not be forgotten that here the derived current is proportional to the principal current, so that its intensity may be regarded as being quite approximately the measure of that of the discharge which traverses the tube.

resistance once surmounted, we may interrupt the passage of the discharge without incurring the necessity of waiting more than an instant for the transmission to recommence, when we close the circuit anew, provided the interruption does not exceed an hour or two. The luminous current presents, with hydrogen under a pressure of 5 or 6^{mm}, very neat and distinct striæ of a rose color; at a pressure of 2^{mm}, they become much larger and less distinct; the color is also paler. The same occurs with air and with nitrogen, but the effects are more striking with hydrogen. A remarkable appearance presented by the current in the interior of the spiral is, that it seems to undergo a very distinct movement of rotation, in a direction which appears to vary with the direction of the discharge; but this last result is not very constant, which has led me to think that the rotation is only apparent, and that it is the effect of the discontinuity of the discharges which constitute the current, a discontinuity which produces the illusion of a displacement. This point, however, deserves to be studied anew.

In order to observe the action of magnetism on the spiral current, I place the spiral of glass between the two poles of the electro-magnet in such a way that its plane shall be the same with that of the two polar surfaces, the two prolongations being thus rendered vertical, the one above, the other below, that plane. The magnetization, according to its direction, either condenses the current towards the interior walls of the spiral tube, or, on the contrary, repels it towards the exterior walls, rendering it very diffuse. In the first case, it becomes highly brilliant, and the stratifications are very distinct; in the second case, they are but slightly visible, and the current itself is much larger and quite dim. It appears to undergo, in even a more sensible manner, the movement of rotation, of which we have spoken. A quite curious fact is, that in the vertical branch of the tube which is below the spiral, and consequently between the two branches of the electro-magnet, the current divides itself, under the influence of the magnetism, into two streams or filaments, which tend, respectively, to one and the other side of the tube. Of these two filaments, one is very small, and of little brilliancy, in comparison with the other. The cause of this separation consists, very probably, in the fact that the inductive current of the Ruhmkorff apparatus is really composed, as we have already taken occasion to say, of two successive and opposite inductive currents, one having much more tension, and passing almost exclusively through the gas, while the other is transmitted with much difficulty, but yet passes, in very small proportion, it is true, since the action of the magnet separates it from the principal current, which is the only one in general that it is requisite to consider in this kind of phenomena, because it is by much the strongest.

I have sought to determine in the case of the spiral tube, as I had done with the large rectilinear tube, the influence of magnetization on the resistance of the gas to the transmission of the discharge, and I have obtained a rather curious result. The two points of platina of the apparatus of derivation being at a distance of ten millimetres from one another in the distilled water, I obtained a derivative current of 20°, the spiral tube being filled with hydrogen under the pressure of 2^{mm}. The spiral was placed vertically between the two horizontal armatures of the electro-magnet which were exactly in contact with its two faces. As soon as the magnetization took place, the derivative current was reduced to 15°, when the discharge was repelled, and driven towards the exterior walls of the spiral with an apparent movement of rotation, and it was raised, on the contrary, to 25°, when the discharge was condensed towards the interior walls of the spiral. Does this influence of the direction of the current or of the magnetization depend on the particular form given to the stream, or to the small diameter of the tube, in comparison with its development in length? It is a point for future elucidation.

I pass now to the case where the magnetic pole is in the midst of the gas which transmits the discharge. I have first operated with a spherical globe,

about fifteen centimetres in diameter, furnished with four tubulures situated at the respective extremities of two diameters of the globe, which intersect one another at right angles. Two cylindrical rods of soft iron are fixed by means of two of these tubulures in the interior of the globe, in the direction of the same diameter, so that their interior extremities may be at a distance of about eight or ten centimetres from one another, while their exterior extremities project from the tubulure nearly two centimetres. It is these exterior extremities which are to be placed in contact with the poles of a strong electro-magnet, in order that the interior extremities may thus become two magnetic poles. The two other tubulures serve to introduce into the interior of the globe two isolated metallic rods, terminated by balls which are at a distance of about ten centimetres from one another, and which serve as electrodes to the electric stream whose direction is thus equatorial, that is to say, perpendicular to the right line which joins the two magnetic poles. As long as the rods of soft iron are not magnetized, the electric stream remains perfectly rectilinear; but so soon as magnetization takes place, the stream, which we will suppose to have a horizontal direction, takes the form of a half circumference of a circle situated either above or below the line which joins the magnetic poles, according to the direction of the magnetization or that of the discharge. The form of the luminous arc is that of a half ring much flattened, as well as widened. The striæ are strongly marked in it, more than they were in the rectilinear current, and its exterior part is much serrated, especially when the gas contains a little vapor of alcohol or ether. If the electric current, instead of being equatorial, is axial, that is to say, directed from one of the magnetic poles to the other, these two poles serving it as electrodes, it experiences no sensible modification under the influence of magnetization.

If, however, the discharge is made to pass between a ball of brass and one of iron, placed at the extremity of an iron rod so as to be capable of being magnetized, there is observed, at the moment of magnetization, a movement of depression, or of elevation in the luminous atmosphere which surrounds the ball of iron. This movement pertains evidently to the change of direction undergone by the electric filaments which radiate from the ball. But the best mode of studying the action of magnetism in the cases where the magnetized bar is in the interior of the gas, is to make use of a bell or cylindrical jar sixteen centimetres in diameter by twenty centimetres in height, in the axis of which is placed a rod of soft iron having a diameter of about three centimetres, whose rounded end is situated at the middle of the axis of the cylinder. This rod is planted in a circular disc, which serves to close the jar. A metallic ring, about twelve centimetres in diameter, formed of wire from 3 to 4^{mm} in diameter, and having for its centre the top of the iron rod, is situated in a plane perpendicular to the axis of the jar. This ring communicates, by means of a rod covered with an isolating coat which is soldered to it, with one of the poles of the Ruhmkorff apparatus, while the other pole is placed in communication, outside the jar, with the extremity of the rod of soft iron, which, in the interior of the jar, is also covered with an isolating coat, except at its summit. It is between this summit and the ring of which it is the centre that the discharge takes place. In order to magnetize the rod of soft iron, it now suffices to place it in contact, by its exterior extremity, with the pole of an electro-magnet, taking care to place between the two a thin strip of caoutchouc to serve as an isolating layer, so that the whole apparatus shall be well isolated. The cylindrical jar is also closed at that one of its two extremities where the rod of soft iron is absent, and it is there furnished with two cocks, of which one serves to form a vacuum, and to introduce a gas which is more or less rarefied; and the other, constructed in Gay Lussac's manner, permits the introduction into the ball of a greater or less quantity of vapor of whatever nature.

I have made many experiments with this jar by filling it successively with atmospheric air, with nitrogen, and with hydrogen, at different degrees of rarefaction, these gases being at times perfectly dry; at others, containing a greater or less proportion of vapor, either of water or of alcohol.

Atmospheric air and nitrogen, when both dry, give nearly identical results, with this difference, that the light is more vivid and clearer with nitrogen. If the soft iron be taken for the positive electrode, and the ring for the negative one, the luminous current is seen to form, at a certain degree of rarefaction, a sort of peach-red envelope around the top of the soft iron, and a sheath of a pale violet color along an arc of a greater or less number of degrees around the ring. At a very weak pressure this sheath encompasses the whole ring, while the top of the soft iron is completely enveloped with a rose-colored aureole, from which issues a very short stream of the same shade, and presenting the form of a large virgule, or comma. This virgule, when the iron is magnetized, is distinctly seen to turn in one or the other direction, with the aureole from which it emanates, according to the direction of the magnetization. The violet-colored sheath which surrounds the ring is also seen to turn in the same direction with the rose-colored aureole, although they are separated by a space completely obscure. By changing the direction of the discharges, there will be seen at the negative electrode a violet-colored envelope, which only covers the whole surface of the top of the iron rod when the gas is very much rarefied, and at the positive electrode, brilliant points, separated from one another by a roseate glimmer which surrounds the entire ring, and whence emanate regular stratifications, internally concentric to the ring. When the gas is not greatly rarefied, there is seen to issue from the ring a luminous jet which tends to the summit of the central rod of soft iron, being only separated from it by a small, black space, and which undergoes a movement of rotation in one direction or the other, like the hand of a watch, according to the direction of the magnetization. In this case there is but a portion of the top of the iron rod which is covered with the violet envelope, and this luminous segment turns with the brilliant jet.

I have made a great number of experiments, under the conditions just indicated, with atmospheric air, with nitrogen, and with hydrogen, whether dry or more or less charged with vapors. I shall proceed to give a description of them in a summary manner, first remarking, however, that, whatever be the gas and its degree of elasticity, whether it be dry or impregnated with vapor, the rapidity of rotation is always much greater when the ring serves as the positive than when it serves as negative electrode, and that this rotation, which increases in rapidity in proportion as the tension diminishes, ceases to be appreciable at a much less tension in the second case than in the first.

In my earlier experiments I had made use of a large globe, twenty-five centimetres in diameter, in which the ring was twenty centimetres in diameter, and the central iron rod three. This globe was furnished with two tubulures, one serving to introduce the iron rod, whose top reached the centre of the globe, and its lower extremity issuing from the tubulure, so as to be capable of resting on the polar surface of an electro-magnet. The other tubulure was closed by a cock, which served to introduce the gas and vapor, and from it there issued an isolated conductor, which supported the ring and admitted of its being placed in the circuit. The discharge thus passed between the summit of the rod of soft iron and the metallic ring.

This globe was filled with air rarefied to 4^{mm}. The discharge took place under the form of a stream which turned with a rapidity of sixty revolutions per minute when the ring was positive, and twenty when it was negative. At a pressure of 6^{mm} the velocity was only forty revolutions per second in the former case and twenty in the latter. With vapor of alcohol, at a pressure of 5^{mm}, the velocity was respectively twenty-two and eleven revolutions per minute.

After these first experiments, which served as my introduction to this sort of researches, I resumed the study by availing myself of the jar of twenty by sixteen centimetres, described above. The following are the results obtained with dry atmospheric air:

Pressure.	Number of revolutions in a minute. *Ring, positive.*	*Ring, negative.*
16^{mm}	55	36
12^{mm}	83	55
9^{mm}	99	63
6^{mm}	,,	100
3^{mm}	,,	128
2^{mm}	,,	,,

At 9^{mm}, with the ring serving as a positive electrode, there is no longer a stream, but a dilatation of the discharge, forming a sector of from 30° to 45°; and this sector obeys the movement of rotation as the stream before obeyed it. But it enlarges, in proportion as the pressure diminishes, and at 6^{mm} forms a complete circular sheet, and it is then that the rotation, which, up to this point, had increased in rapidity, becomes no longer sensible. When the ring serves as a negative electrode it is covered with a violet sheath, whose size likewise increases in proportion as the pressure diminishes, but which occupies only half the circumference of the ring under a pressure of 4^{mm}. It is seen to turn very rapidly, but at a pressure of 2^{mm} it occupies the whole circumference of the ring, and there is no longer any sensible rotation. At the summit of the magnetized iron rod there is a roseate aureole, from which, as has been said, emanates at one point a very short jet in shape of a comma, which turns with the violet-colored sheath, from which it is separated by a very considerable obscure interval.

It should be remarked that, at a pressure of 6, of 4, and sometimes of even 3^{mm}, it most often happens, when the ring serves as positive electrode, that at the first moment of the circuit being formed there issues a stream which turns too rapidly to allow its velocity of rotation to be measured, but which quickly expands so as to form, for some instants, a sector which continues to revolve, and soon after a complete circular sheet, which no longer manifests any movement.

It does not follow that the action of magnetism is annulled when the gas is too much rarefied for the continuance of a sensible rotation. That action is manifested under another form, as is shown by experiments made under a pressure of from 3 to 2^{mm}. Thus, if the ring serves as negative electrode, the violet sheath which surrounds the soft iron is seen, at the moment when this last is magnetized, to subside sensibly, and to rise at the instant of its being demagnetized. If, on the contrary, the ring serves as positive electrode, the rose-colored sheet which fills the interval between the ring and the summit of the central rod of iron is raised, as well as the violet sheet which issues from that summit, at the moment of magnetization, and depressed at the instant of demagnetization.

The following is a more complete experiment with *dry nitrogen*, and shows that rotation begins to manifest itself at stronger pressures when the ring is positive than when it is negative:

Pressure.	Number of rotations in a minute. *Ring, positive.*	*Ring, negative.*
35^{mm}	12	,,
29^{mm}	27	,,
21^{mm}	45	36
16^{mm}	67	51
12^{mm}	99	59
8^{mm}	115	70
6^{mm}	,,	115
5^{mm}	,,	150

At 4^{mm} the rotation is too rapid to allow its degree to be observed; at 3^{mm} it appears completely to cease. The rose-colored aureole is very vivid when the summit of the rod of soft iron is positive. When there is no longer any rotation, there is observed, as with atmospheric air, a movement of depression and of ascension under the influence of magnetization.

The presence of vapor modifies in some important particulars the results obtained with dry gases. The following is an experiment made with ordinary air subjected to a pressure of 2^{mm}, into which vapor of water has been introduced in successive quantities, so as to increase that pressure solely by the effect of the presence of the vapor:

Pressure.	Number of rotations in a minute.	
	Ring, positive.	*Ring, negative.*
2^{mm}	,,	,,
4^{mm}	,,	,,
6^{mm}	,,	92
8^{mm}	140	70
10^{mm}	120	52
12^{mm}	90	50
14^{mm}	80	48

We see that at an equal pressure the rapidity of rotation is greater with vapor of water than with dry air, which is attributable probably to the greater facility with which the electric discharge is transmitted. With the external air of a mean humidity, we have, with a pressure of 14^{mm}, 72 revolutions instead of 80 when the ring is positive, and 44 instead of 48 when it is negative.

But the most characteristic fact produced by the presence of watery vapor is the division, under the influence of magnetism, of the single current into several small distinct and equidistant currents, which turn like the radii of a wheel. This division is only observed when the ring serves as a positive electrode. At a pressure of 6^{mm} the single current begins with turning, then expands, whereupon the rotation is no longer perceptible; but at the pressure of 8, of 10, and of 12^{mm} this current, from the commencement of its rotation under the action of magnetism, divides into five or six small streams which turn, as was just said, like the radii of a wheel; while, when the air is dry, the current never divides; but, under a weak pressure, it merely expands into a sector or a circle of which all the parts are continuous.

When the ring is negative, and there is vapor present, it will be seen that the current which issues from the summit of the iron rod presents, where it is in contact with the iron and at the moment when this is magnetized, instead of a continuous surface, a series of small brilliant points, which seem points of emanation for as many small currents, too little distant from one another to become distinct. Here, then, this current, which does not divide into separate filaments, simply undergoes dilatation or expansion at the point where it is in contact with the iron.

The vapor of alcohol produces similar effects with the vapor of water. The single current is, in this case, much more brilliant than with dry air or with the vapor of water; it presents fine stratifications, which give it an appearance not unlike that of a caterpillar. Magnetization expands and divides it into several currents, sensibly larger than those observed with the vapor of water. If, however, the diameter of the ring is too large, greater, for instance, than fifteen centimetres, the subdivision of the current is not effected without difficulty, unless the intensity of the discharge and that of the magnetization be very considerable.

The following is an experiment in which, the rarefied gas being hydrogen, different portions of alcoholic vapor were successively introduced. The pressure

of the dry and pure gas was in the commencement 5^{mm}; at this pressure, as we shall forthwith see, the hydrogen transmits the discharge only under the form of a luminous sheet. The pressure was afterwards augmented solely by means of the vapor of alcohol, with the following results:

Pressure.	Number of revolutions in a minute.	
	Ring, positive.	*Ring, negative.*
7^{mm}	Luminous sheet	92
10^{mm}	80	52
12^{mm}	64	48
15^{mm}	48	38
18^{mm}	40	32
22^{mm}	30	25
27^{mm}	24	18
36^{mm}	12	10
38^{mm}	12	10

The division into distinct currents, more or less numerous, was manifested when the ring was the positive electrode.

When pure and dry hydrogen is adopted as the medium in which the discharges take effect, the phenomena of rotation are obtained with great difficulty. At rather strong pressures, such as that of 128^{mm}, we have a number of currents, but these currents are too intermittent to allow of the magnet's acting upon them. At 90^{mm} I have obtained a small stream under the form of a bluish-white filament, which, the ring being positive, turned at the rate of thirty-five times per minute; but, at the lapse of some instants, it became subdivided into a multitude of small, irregular streams, and rotation was no longer perceptible. As far as 40^{mm}, the action of the magnet was indistinct. At 30^{mm}, the negative ring was covered with small violet sheaths, at equal intervals, which seemed to experience, at the moment it was magnetized, a tendency to move in one direction or the other, according to the direction of the magnetization. The same was the case with the small brilliant points, likewise distributed at minute intervals, with which the ring, when positive, is covered. At 5^{mm}, and still more at three and at two, the ring is entirely covered, when it is negative, with a fine violet-colored sheath, which becomes contracted under the influence of the magnet. The top of the iron rod, which is then positive, is surrounded by a beautiful white aureole, slightly tinged with rose, three centimetres in breadth, and stratified in a very marked degree. Magnetization sensibly contracts this aureole, and compresses its striæ without diminishing their number, elevating it, and, at the same time, giving it the form of a pear resting with its base on the magnetic pole. When this pole is the negative electrode, there issues from it, as we have seen, a brilliant tuft of a violet color, which conforms itself to the action of the magnet.

All the phenomena just described show, in a striking manner, the molecular differences which various elastic fluids present, as regards one another, even at an advanced degree of rarefaction. Thus in hydrogen, although that gas is a very good conductor of electricity, electric currents can, with difficulty, and, indeed, scarcely at all, obey the action of the magnet, probably by reason of the slight density of the gas. In air, and in nitrogen, it is quite otherwise, and still more when these gases are humid. The singular property possessed by the electric current of dividing itself into several small and distinct streams, instead of diffusing itself, under the influence of magnetization, when the medium which transmits it contains a more or less quantity of vapor, would seem to indicate in the vapor a greater cohesion than in the gases properly so called, if, indeed, we may employ the term cohesion when the question relates to elastic fluids so much rarefied. It might also be possible that this division into streamlets is

the result of an optical illusion, due to a very rapid succession of jets emanating from different points, and which, in reality, are not simultaneous. This is a point for examination.

However this may be, it is evident that the study of the stratification of electric light, and of the action of the magnet on the discharges in different gaseous mediums, discloses differences between those mediums which can only result from their difference of molecular constitution. Density, in particular, would appear to have a great influence on this order of phenomena, since we see hydrogen manifest them in so feeble a degree, while the vapors of water, and especially of alcohol and ether, present them in so decided a manner. The proper nature of elastic fluids, opposing more or less resistance to the transmission of electricity, must, doubtless, also play its part. It might not be impossible then, that, in a more detailed and more exhaustive study of the phenomena with which our attention has been occupied, and more particularly of those relating to the action of the magnet on electric currents propagated in much rarefied elastic fluids, we may be able to find the means of acquiring some new ideas on the physical constitution of bodies, and on the manner in which the propagation of electricity is therein effected.

REPORT ON THE PROCEEDINGS

OF THE

SOCIETY OF PHYSICS AND NATURAL HISTORY OF GENEVA,

FROM JULY, 1862, TO JUNE, 1863.

BY PROFESSOR MARCET, PRESIDENT.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION.

In proceeding, as has been the custom of my predecessors, to present an account of the labors of the society during the year just elapsed, it is but proper that I should acknowledge how greatly my task has been facilitated by the scrupulous exactness with which the reports of our several meetings have been drawn up by our secretary, M. Ed. Claparède. Among the topics claiming my attention, many have been already communicated to the public, or are about to be so, through the medium of scientific journals; as regards these, therefore, I shall restrict myself to an indication of the titles, or a very summary analysis of the conclusions arrived at. In the arrangement of subjects I cannot do better than adopt the division into two sections, that of the physical and that of the natural sciences, first proposed by M. de la Rive, and since observed by the greater part of the presidents who have succeeded him. I shall follow, moreover, the example of my immediate predecessor in touching very lightly on the discussions which have taken place either on the occasion of original memoirs read before the society or of verbal reports on recent discoveries made in other countries; not that these discussions have not often possessed a genuine interest, but because it is essential, if this valuab'e observance is to be retained by us, that the appreciation of the labors of others, the verbal communications in which one is sometimes led to enunciate ideas arising at the moment and perhaps not always sufficiently considered, should receive no greater publicity than that which results from the reading of the journal of our sittings.

PHYSICAL SCIENCES.

Our indefatigable colleague, Professor Gautier, has continued to keep the society well informed of the discoveries made in *astronomy*. His communications have been numerous and diversified; we must here limit ourselves to the mention of the most important. M. Gautier presented to the society, in the first place, a report on the observations of M. d'Arrest, of Copenhagen, relative to the number and to the variability in brightness of the nebulæ, as well as to certain points, still doubtful, which would tend to indicate a proper movement in some of those bodies; secondly, an account of a memoir of M. Lamon on the periods of the variations of magnetic declination, and the analysis of researches by M. Maine on the flattening of Mars, which he estimates at $\frac{1}{39}$; thirdly, a report on some recent observations of M. Donati on the comets, and on a memoir of the same author relative to stellar spectra: M. Gautier announced on this

occasion that Father Secchi also was occupied in the study of stellar spectra compared with the solar spectrum; fourthly, M. Gautier presented lastly to the society a plate of Father Secchi, representing the different appearances of the nucleus of the comet of 1862, differences which, as M. Wartmann, sr., has pointed out, might result, at least in part, from the circumstance that the observations took place at different hours.

Professor Plantamour announces that he has collated the series of observations made for twenty years on the latitude of the observatory of Geneva. That latitude would be 46° 11′ 58″.75, with a mean error of some hundredths of a second.

Meteorology and *terrestrial physics* establishing a natural bond between astronomy and physics properly so called, we shall first direct our attention to several communications which we owe to Professor Plantamour. Besides the annual meteorological summary for Geneva and Saint-Bernard, published, as usual, in the archives of the physical and natural sciences of the "Bibliotheque Universelle," M. Plantamour has communicated to the society an interesting memoir relative to observations made at Geneva, for thirty-five years, on the force and direction of the winds. He has found that in winter the number of northeast and that of southwest winds balance each other; the northeast predominates in spring and in autumn, the southwest in summer. The general resultant is a little west of north, which proceeds from the fact that the mean direction of northeast winds more nearly approximates to north than does the mean direction of southwest winds to south. The above results are somewhat modified if the origin of the winds be taken into account and if local are distinguished from general winds. The former depend chiefly on the vicinity of the lake and the variation of temperature in the twenty-four hours, giving rise to a regular breeze morning and evening, analogous to breezes of the land and sea. The memoir of M. Plantamour has been lately published in his extensive work on the climate of Geneva. (See page 15, *et sequent.*)

The same savant read to the society a memoir on the diurnal variations of the atmospheric pressure, a memoir likewise published in the work just mentioned. After having passed in review and combated as insufficient the theories proposed by MM. Krail and Dové, M. Plantamour concludes in favor of that proposed by M. Lamon, according to which the phenomenon of the diurnal variation would depend on two distinct influences, one resulting from the temperature properly so called, the other from a kind of electric attraction, whose nature is as yet completely unknown, but owing probably to the action of the sun. M. Plantamour founds his preference for this theory over the preceding on the consideration that it furnishes the means of explaining the double diurnal oscillation which is observed in the barometer, while the influence of the temperature, it would appear, ought to produce but a single one. The author presents, in support of his opinion, a comparative table of the diurnal variation of the temperature and of the barometer for Geneva and Saint-Bernard.

To complete our analysis of what relates to terrestrial physics and meteorology, I have still to notice two communications, one from Professor de la Rive, relative to an aurora borealis observed in the month of December, in which the rotation of the arch from east to west was perfectly evident, and another from M. Louis Soret, who has presented to the Society an apparatus constructed according to his directions in the workshop of M. Schwerdt, an apparatus designed for the measurement of heights by a determination of the temperature of the ebullition of water. In the construction of this instrument, the chief object of M. Soret has been to attain a perfect precision in thermometrical indications, a condition which has heretofore been wanting. He has succeeded, on the one hand, by surrounding the ball of the thermometer with two envelopes of vapor instead of one, in abating the variations of temperature proceeding from without; and, on the other hand, he prevents the effect of an ebullition often too

much precipitated, by immersing the bottom of the lamp of alcohol, by the flame of which the water is to be made to boil, in a bath of cold water. The sole, yet somewhat grave objection which has been advanced against this apparatus, is, that in a still greater degree perhaps than the barometer, it requires to be observed with scrupulous care, and demands precautions which can scarcely be expected on the part of observers who are not physicists.

If we pass now to *physics* properly so called, we shall see that, as in the past, it is electricity which has played the principal part in the communications made to the Society during the year under review. Our colleague, M. de la Rive, has communicated to us, at two consecutive meetings, the results of his researches on the phenomena which characterize and accompany the propagation of electricity in highly rarefied elastic fluids. In the classification of his apparatus, M. de la Rive insists more particularly on the means which he has employed to measure the intensity of the discharges or transmitted currents, by availing himself of a derived current taken by means of two small sounds of platina, in the distilled water placed in the circuit of the principal current. He also describes a manometer which enables him to appreciate to nearly the fiftieth part of a millimetre, and, for practiced eyes, even to the hundredth part, the tension of the elastic fluid submitted to experiment. The researches of M. de la Rive have been directed to atmospheric air, nitrogen, and hydrogen. He has studied, in the case of each of these gases, the influence of the dimensions and form of the gaseous mass, as well as of the pressure, on its capacity for transmitting electricity. He has described the successive appearances which the electric light assumes, in proportion as the pressure of the gas diminishes, and particularly the variable form and size of the stratifications of that light, together with the formation of a violet-colored photosphere around the ball serving as a negative electrode, and of a black space, from five to ten centimetres in length, which separates that photosphere from the stratified luminous column. He has satisfied himself, in the course of a great number of experiments, that these appearances of electric light in rarefied gases are due to a mechanical effect produced by the transmission of electricity, an idea which had already been advanced by M. Riess. M. de la Rive has succeeded in showing, by direct experiments, that the mechanical effect in question consists in a considerable dilatation of the gaseous matter near the negative electrode, followed by alternate contractions and dilatations in the column up to the positive electrode. First. He was easily able to verify, by means of the manometer, the existence of the oscillatory movement in the gaseous column, and the variations in its intensity, which depends, as he has shown, on the nature, degree of tension, and dimensions of the gaseous mass in question. Secondly. He has demonstrated experimentally that if, by means of small sounds of platina suitably arranged, derived currents are taken in different parts of the luminous column, all traversed by the same discharge, great differences will be found in the intensity of these currents, differences which prove that the obscure parts possess a greater conducting capacity, and are consequently the most dilated. With hydrogen, the best conductor of the gases, no derived current is obtained in the obscure part of the column. Thirdly. M. de la Rive points out that the indications of the thermometer placed in different parts of the stratified column conduct us to the same results, by evincing great differences between the temperatures of those different parts; the more obscure parts being sensibly less warm than the luminous, which proves that the former are better conductors. The author has obtained a great number of numerical results, indicating the differences of temperature, at different pressures of various portions of the gaseous column traversed by the discharges.

M. de la Rive completed his communication at a subsequent session, by explaining to the Society the modifications produced in the phenomena relative to the propagation of electricity, through highly rarefied mediums, by the action of

a strong magnetic power. This action tends to augment the resistance of the gaseous substance to the transmission of electricity, by condensing the gaseous filaments, and has in particular the effect of rendering luminous the obscure part of the column, by contracting the previously too much dilated gas which occurs there. Lastly, the attention of M. de la Rive was especially drawn to the rotatory and expansive action of magnetism on the electric discharge. He has succeeded in obtaining, in regard to this point, certain very constant facts, such as those relative to the duration of the rotatory movement of the discharge, which varies with the direction of the current, the nature of the gas, and its degree of density. He has also remarked the very great difference which the phenomenon presents, according as the rarefied gas is dry or contains vapor of water or of alcohol. In the first case, the luminous discharge expands under the influence of magnetism into a sheet which forms the surface of a sector, or even that of a full circle when the gas is very much rarefied. In the case in which vapor is present in the rarefied gas, the discharge, instead of expanding, divides into a greater or less number of small partial jets at equal interspaces, forming, as it were, a star animated by a movement of rotation around its centre. These phenomena, and others of the same kind, have led M. de la Rive to establish a difference between permanent gases and vapors, in reference to the point of cohesion, or rather their molecular constitution. The author terminated the reading of his memoir with some general considerations on this extensive subject; announcing that, for the present, conclusions too absolute would be premature, and that he abstains from presenting them until he shall have completed his researches by extending them to a greater number of gaseous substances.

If we have enlarged a little more than is usual in an analysis of the memoir of M. de la Rive, we find a justification, not only in the importance of the subject, but in the circumstance that the results which he obtained have been heretofore published only in fragments. The entire memoir is about to appear in the seventeenth volume of the *Memoirs* of the Society, now in the press.*

It should be added, that we owe to M. de la Rive the model of a new system of Grove's apparatus. The modification which he has introduced into the battery of that physicist is essentially calculated to render its management more commodious and prompt. His instrument, which is extremely manageable, and is furnished with conductors of alumina, possesses the advantage of requiring little manipulation, and of rendering superfluous the removal of the nitric acid; the same acid suffices for the service of several days and many experiments. With the help of a single pair of this battery, M. de la Rive has been able to repeat all the principal experiments of the electro-dynamics of Ampére—experiments which usually require five or six pairs of Grove or of Bunsen.

Besides some verbal communications by Professor Wartmann relative to electrical phenomena, particularly to the limit of pressure which permits a spark to pass through a gaseous medium, as well as to the influence which the state of tension of a gaseous medium exercises on the passage of a current, the savant just named engaged the attention of the society by an account of some of the principal subjects discussed in the last reunion of the British Association at Cambridge, at which he was present. Among the communications made on that occasion, M. Wartmann cites more particularly the observations of M. Nasmith relative to the structure of the sun. To avoid the inconvenience of a too great light, M. Nasmith, instead of introducing the solar rays directly into the eye, places near the object glass a lens which is plane on the side next the eye, but concave on the opposite side, so as to disperse the luminous rays and allow the study of only the quantity of light reflected by the plane surface. The author has thus been able to ascertain that towards the hour of noonday

* A full translation of this interesting memoir is given in this report.—See page ——

the luminous envelope of the sun presents a great number of spindle-shaped images, which might be compared to willow leaves strewn confusedly over its surface. Of these M. Wartmann has presented to the Society a photograph taken from the original designs of M. Nasmith. These images seem to be displaced one by another, sometimes parallel to their axis, sometimes by an angular movement. The preceding observations have been confirmed by M. Pritchard, who announces that they may be repeated with a good telescope of from three to four inches. M. Wartmann also gave an account of experiments in telegraphic electricity by M. Wheatstone, which he witnessed, and by which it is practicable to obtain despatches written with extraordinary rapidity.

The same physicist communicated to the Society a note relative to an electrical phenomenon observed by M. Alizier, teacher at Geneva, July 24, 1856, on the summit of the Oldenhorn. Of a sudden the staves borne by M. Alizier and the persons who accompanied him began to sound in the manner of the posts of the telegraph. In a few moments a heavy storm of hail descended.

Professor de la Rive, on his return, in May, 1863, from a sojourn in Paris, reported to the Society several new scientific facts which he had gathered. He drew attention, in particular, to an investigation of M. Helmholtz, by which that savant had arrived, simultaneously with M. W. Thompson, at the conclusion that the earth cannot be liquid in its interior. He also thinks himself entitled to affirm that it is not necessary to recur to the hypothesis of aerolites falling continually into the sun, in order to explain the persistence of the high temperature of that body. It suffices to admit that the sun, having become heated by an undetermined cause, is now growing cold with extreme slowness; for, according to M. Helmholtz, the calculations heretofore made greatly exaggerated the rapidity of refrigeration in regard to that body, because they neglected to take account of an important element, namely, that the sun diminishes in volume as it grows cooler, and that this contraction must develop new heat.

M. de la Rive presented to the Society, in the name of his son, M. Lucien de la Rive, a memoir on the number of independent equations in the solution of a system of linear currents. This memoir, being wholly mathematical, is not adapted to analysis.

Professor Marcet has continued to impart to the Society many facts relative to nocturnal radiation; among others, to an altogether abnormal refrigeration of the surface of the ground, and of the stratum of air in immediate contact with it, which he has remarked during the first days of March in localities turned towards the north, not only at the hour of sunset, but even during the warmest hours of the day. The author attributes this extraordinary cooling of the surface to the concurrence of several atmospheric circumstances, but more especially to the extreme dryness which had prevailed for some time, and which, as Tyndall has proved, peculiarly facilitates the radiation of terrestrial heat.*

M. Marcet has taken advantage of the residence of his son in Australia, to induce him to repeat at Queensland, under the 22d degree of south latitude, the experiments on nocturnal radiation, which have been recently made in our temperate climates. It would seem to result from these experiments that the phenomenon of the increase of temperature at certain periods of the day, when we ascend some feet above the surface of the earth—a phenomenon so well authenticated in our temperate climates—is not remarked in the regions of the torrid zone either at the rising or setting of the sun; or if it takes place, it is in a degree scarcely sensible, hardly ever exceeding $0^{\circ}.4$ Cent. M. Lucien de la Rive has recently made some observations in Egypt, on the banks of the Nile, which would appear to lead to an analogous result. M. Marcet explains

* See *Archives des Sciences Physiques et Naturelles*, April, 1863.

this apparent anomaly by attributing it to several causes, but more particularly to the great quantity of water, under the form of elastic vapor, held by the atmosphere in tropical regions, especially in countries but little remote from the sea—vapor which, it is known, possesses the property of intercepting in a high degree the dark heat emitted by the ground, and which would thus contribute to render so much less apparent the effects produced by the nocturnal radiation.

Communications on *chemistry* proper have this year been less numerous than usual. We have scarcely anything to cite but some remarkable researches of Professor Marignac on the tungstates, the fluo-tungstates, and the fluoborates. The subject, although of great importance, and treated in a masterly manner, is too special to allow of my presenting here even a summary analysis. We may, besides, direct the reader for a detailed extract of the memoir to the *comptes rendus* of the Academy of Sciences, in anticipation of its appearance *in extenso* in early numbers of the *Annales de Chimie et de Physique.*

Dr. W. Marcet has drawn the attention of the Society to investigations made by him on the digestion of fats, particularly on the mode in which the emulsion of those substances is effected by means of the bile, and probably also of the phosphates, which occur abundantly in animal food. The same chemist also communicates experiments, which he has recently undertaken, on the composition of the gastric juice, and on the changes which it undergoes as to the degree of acidity during the act of digestion.

NATURAL SCIENCES.

The natural sciences, and more especially *geology* and *paleontology*, have this year had a large share in the labors of the Society. We should mention, in the first place, several important communications of Professor A. Favre; and, first, his geological chart of portions of Savoy, Piedmont, and Switzerland, in the neighborhood of Mont-Blanc—a chart drawn on a scale of $\frac{1}{150000}$, and which is the result of persevering and conscientious labors pursued since 1840. M. Favre has also presented us with the geological chart of the Jura mountains pertaining to Basle—the first published at the expense of the confederation, under the care and direction of M. Müller. It is designed on a scale of $\frac{1}{50000}$. There is reason to fear, however, that the enterprise cannot be continued in such wide proportions, and that it will be necessary to return to the scale of $\frac{1}{100000}$. The chart is accompanied by a publication in two series—one for the Jura, the other for the Alps.

M. Favre also read to the Society a memoir containing a detailed description of the mountain of the Voirons, of which he has determined the succession of the different strata. This memoir will soon appear in the text which will accompany the chart of Savoy.

The same geologist read to the Society a critical analysis of MM. Koechlin-Schlumberger and Schimper on the transition deposit of the Vosges—a deposit referred at present to the old carboniferous series. He also presented, in the name of M. Studer, a geological memoir on the Balligstock and the Béatenberg, situated on the borders of the Lake of Thoune—a memoir which has been published in the Archives of the Physical and Natural Sciences.

Professor Pictet read to the Society a note containing critical observations on the subject of a new stratum, which M. Coquand proposes to introduce into the series of cretaceous formations—a stratum already known under the name of "alpine neocomian," and to which he proposes to give that of "*barémian*," considering it as the equivalent of the yellow stone of Neuchâtel. M. Pictet, without disputing the propriety of a new name, does not admit, between the barémian and the yellow stone of Neuchâtel, so precise and restricted a parallelism.

The same savant called the attention of the Society to an alleged reptile with feathers, found in the jurassic of Solenhofer, and described by M. Wagner as possessing at once the tail of a reptile and the feathers and feet of a bird. This fossil has been acquired for the British Museum by M. Owen, who will soon publish a detailed description of it.

The Society has continued to keep itself informed of the facts relative to the "*fossil man*." Its interest has been particularly excited by the discovery of the human jaw-bone of Moulin-Quignon, near Abbeville. M. Pictet, who took occasion very recently to study, at Paris, this bone, and the hatchets which accompanied it, has set forth to the Society the reasons which seemed to him to render the authenticity of those objects incontestable, notwithstanding the doubts at first expressed on this subject by eminent paleontologists. More recently we have learned with much interest that a sort of scientific congress had been convoked at Paris, and the authenticity admitted with unanimity. It remains to solve the question of antiquity—that is to say, to decide what place the deposit of Moulin-Quignon should occupy in the series of quaternary and modern formations.

M. Renevier has communicated to us a photographic view of the Diablerets, geologically colored, and has, at the same time, given to the Society an account of some recent geological excursions in the vaudese Alps. He has been enabled to complete the series of jurassic formations in this district by the discovery, in the Diablerets, of a stratum of *bajocian*, (inferior oolite,) and of a stratum of *bathonian*, (greater oolite,) the first being characterized by a gigantic fucoid. Finally, M. Renevier announces grains of "chara" in the nummulitic of the Diablerets.

We arrive now at organic natural history, and it remains to speak of botany and zoölogy.

Botany.—Professor De Candolle has presented to the Society several interesting communications relative to vegetable physiology and to botany proper; particularly a paper on a new character observed in the fruit of oaks, and on the best division to adopt for the genus "Quercus;" a memoir entitled *Studies on species, occasioned by a revision of the family of Cupuliferæ,* in which the author discusses the system of Darwin, and the theory, applied to the vegetable kingdom, of a succession of forms proceeding from the deviations of an anterior form. Both these memoirs having been published in the archives of the physical and natural sciences of the *Bibliotheque Universelle* we shall here content ourselves with indicating them to savants who are interested in questions of this kind.

Besides the original memoirs just cited, M. de Candolle brought to the notice of the Society some interesting results of observations made by M. Schubler "on plants cultivated in Norway." The author has shown us in what degree the deficiency of heat, in northern regions, appears to be compensated by the prolonged action of the light due to the length of the days; to such an extent that, in proportion as we advance towards the north, the coloration and sapidity of plants seem to increase rather than diminish in intensity.

M. de Candolle has also drawn attention to two memoirs of Dr. Hooker. The first relates to a plant discovered on the African continent, opposite Fernando-Po, to which he has given the name of *Welwitschia*. This plant, whose trunk is a cone of little height, surmounted by a torous *(bosselé)* table attaining a diameter of six feet, presents the singular character of having but two leaves, which are indeciduous cotyledons. It is the only vegetable known whose cotyledons are not caducous. The second memoir of M. Hooker relates to the celebrated group of cedars of Lebanon, which is found to be established on the moraine of an ancient glacier, and which this botanist visited in 1860. M. Hooker is inclined to think that, in the present circumstances of climate, this tree could, with difficulty, establish itself on the mountain where it is found, and pronounces

the opinion that the old cedars which now exist there are but the remains of an ancient forest, dating from an epoch more favorable to the development of the species. It is certain, in the mean time, that the cedar of Lebanon, that of the Himalaya, and that of the Atlas present varieties which it is difficult to distinguish from one another. Hence M. Hooker is disposed to admit that they all descend from one primitive form, which has spread itself over a vast region when the climate was more temperate than it is at present.

To Rev. M. Duby we are indebted for a note relative to observations made at Bombay, on a champignon or fungus which attacks the feet of the natives, and produces a malady known in the country under the name of "podelcoma mycetoma." The bones of the foot and lower leg are gradually perforated through and through, and the champignon, which bears spores very similar to those of the oïdium, lodges in the cavities thus formed, under the shape of a spongy mass. M. Duby has also occupied our attention with the very ingenious observations of M. Darwin on "the mode of fecundation of the red flax." The same botanist also announces that he has observed in the *Callistachys linearis* a very remarkable movement of the inferior leaves which, at the decline of day embrace the stem, while the superior leaves embrace the ear.

Zoölogy and Physiology.—Dr. Dor called the attention of the Society to a new theory of Daltonism, or rather to an old theory of Young, to which there seems to be a tendency to recur at the present time. Agreeably to this theory there exist in the retina three descriptions of nervous fibres; the first sensitive to red, the second to green, the third to violet. Daltonists, then, would be those in whom one of these orders of fibres is completely paralyzed. M. Dor has also proposed a new scale of characters for measuring the distinctness of vision.

M. Victor Fatio presented to the Society a specimen of a lizard of the Alps called "Lacerta nigra," regarded by some authors as constituting a particular species. M. Fatio is rather disposed to consider it as being but a simple variety of the "Lacerta vivipara," and he adduces the reasons which lead him to hold this opinion.

The same physiologist read to the Society a note on the habits of the "pléobate cultripede," of the coasts of Brittany. He has ascertained that this batrachian is a nocturnal animal, which buries itself during the day in the sand, and remains there till night in a state of complete immobility. M. Fatio has also communicated to us a plan of geographical distribution, designed to form the basis of an extensive work, which he has undertaken with the view of making a complete catalogue of the vertebrata of Switzerland.

To complete what we have to say on organic natural history, we should mention an interesting notice by M. Muller, relative to the recent modifications which the theory of cellular organization has undergone through the influence of the labors of MM. Brücke and Max. Schultze; and a communication of M. Claparede, in which that physiologist renders an account of some epidemic instances of "trichinus spiralis" lately authenticated in Germany, and more especially in Saxony. It is now known that the larva of this parasite continues to live in the flesh of the hog when insufficiently smoked. Now, a single pair of these animalcules, arriving at maturity in the human intestine, suffice to infect with larvæ all the muscles of the body, and to occasion the gravest consequences, sometimes even death. The danger of such an infection is now so fully realized that the inhabitants of Planen, in Saxony, have established at their slaughter-house an official, provided with a microscope, and have prohibited the sale of hogs whose flesh has not been previously examined with the help of that instrument.*

* For an appendix to this part of the report see the end of this article.

Dr. Gosse has communicated to the Society a note of M. Campbell relative to the frequency of goitre, in the districts near the foot of the Himalaya—a malady with which also goats and sheep are frequently infected when they descend from the mountain Lastly, Dr. Lombard has read to us a detailed extract of observations published by M. Jordannet, a French physician, on the climate of Mexico, considered in a medical point of vie

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Having thus presented a cursory review of our proceedings during the past year, my task unfortunately is still incomplete; for, notwithstanding the restricted number of our members, scarcely a year passes in which your presiding officer, in his annual report, is not called on to deplore the loss of one or more of them. This year has removed two from among us: one of them, M. Le Royer, a retired member, of advanced age; the other, M. Etienne Melly, a member in ordinary, whose years authorized us to hope that we might long retain him. I must not close this report without briefly recalling the titles they possessed to the esteem of the learned world and the affection of their colleagues.

Etienne Melly, born at Geneva, in 1807, early evinced a decided taste for the physical sciences. After successfully pursuing the course of our Academy, he went to Paris to complete his scientific studies, and on his return to his country was attached to the Industrial school of this city as a teacher of physics and chemistry, the study of which he may be said to have created in the establishment in question, and from the superintendence of which he never desisted until the infirm state of his health made it impossible for him to give to his duties the care and attention which his scrupulous conscience exacted. While thus employed he prosecuted divers physico-chemical researches of great interest, only a part of which, owing to his characteristic diffidence, have been communicated to the public. His two principal publications appeared, the first, in 1839, in the *Bibliotheque Universelle*, the second, in 1841, in the first volume of the *Archives de l'Electricité*. The former treats of certain felicitous attempts which he had made to apply platina to other metals by means of pressure so as to obtain a very solid plate, and be thus able to substitute, in certain chemical processes, for utensils of platina, utensils of platinized copper. This mode of platinizing offers greater assurance than that by electricity; in that it better resists the action of chemical agents.

The second publication of M. Melly, and that of most importance, embraces two distinct parts: the first, relating to a more economical construction of the battery of Grove, then just invented, and to the study of the chemical effects of electricity by means of that apparatus. The second part has for its object the study of the chemical effects of the electric spark, whether produced by Grove's battery or by currents of induction. M .Melly sets forth in his memoir the numerous experiments by which he had succeeded in decomposing, by means of that spark, not only distilled water, but the most isolating substances, such as oils, ethers, alcohol, &c. He establishes, by a well-sustained analysis of the results he had obtained, the difference which exists between this mode of decomposition and electro-chemical decomposition properly so called, and he shows that it is an effect, not of electricity itself, but of the intense heat developed by the electric spark.

We know that this decomposing power of heat, carried to a high degree, has been since demonstrated in a direct manner upon water, without the intervention of electricity, by M. Grove, and has been extended upon a wide scale to a multitude of substances by M. Deville, who has called it, "the dissociation of bodies by heat." Still, there will remain to M. Melly the honor of having first, by his ingenious experiments, called the attention of the learned world to this important subject. Independently of what he has made known by his publi-

cations, Melly, who knew no remission of labor, often obtained interesting results which he kept to himself, or communicated but to a few of his friends. The distressing state of his health having compelled him, many years since, to abandon his laboratory, he did not give way to discouragement, but continued to devote himself with the same ardor to the microscopic investigations which constituted the scientific interest of his latter days. Of these he has left but few written notices; their results are contained in his collections, especially in that of the Diatomeæ, of which he has left more than fifty boxes, containing as well the Diatomeæ of the environs of Geneva, as those of foreign lands and those of types determined by known authors. As to the microscopes of which he availed himself, it may be affirmed that never have the Algæ of our country been studied with the help of instruments so perfect. Melly, besides, brought an extreme carefulness to the preparation of microscopic objects; we may judge of it by the following fact reported by Professor Thury in the interesting notice which he read of his friend: The collection of Diatomeæ was twice resumed entirely anew by Melly, because the distilled water and alcohol which he had employed were found to be not absolutely pure.

Of a conversation as frank as amiable, Melly had, moreover, that devotedness for others, whose character is the most complete self-abnegation. Happy in the success and welfare of his friends, every feeling of envy and jealousy was so alien from his nature, that he would not even admit the existence of these evil sentiments in another. Having suffered in his dearest affections by the loss of a beloved consort, he remained thenceforward completely isolated. But this isolation, far from rendering him egoistic, had still more enlarged his heart. His gratitude, for the cares and attentions of which he was the object on the part of his friends was as touching as amiable. The religious sentiments which sustained him in the midst of trials so various and afflicting were always united in him with a perfect tolerance in regard to those who did not share his opinions. It was the fruit of an elevated and disinterested nature, such as is rarely witnessed. He sank, February 4, 1863, after long and acute sufferings.

Auguste Le Royer sprung from an honorable family, and whose ancestors had been pharmaceutists from father to son; was born at Geneva, in 1793. After pursuing his earlier studies in his native city, he went in 1811 to Strasburg, where he passed eighteen months of preparation in studying pharmacy, his future vocation. In 1813 he returned to Geneva, took an active part in the political events of the time, and in 1817 was admitted a pharmaceutist after an honorable examination. Thenceforward Le Royer zealously occupied himself in scientific labors related to his profession. It was in 1818 that the illustrious Dumas, then ten years of age, entered himself as a clerk with Le Royer, and subsequently became his principal assistant. Besides these friendly connexions with Dumas, Le Royer contracted others with Dr. Prevost, taking part in many of the physiological researches of the latter in their chemical bearing. In 1821 he was adopted as a member of this Society and of the Helvetic Society of natural sciences. The departure of M. Dumas for Paris, in 1823, compelled Le Royer to occupy himself almost exclusively with pharmacy, and I know not that he has published anything since 1824 Nevertheless, he preserved a taste for study, and always encouraged the scientific labors of those who approached him. Like Etienne Melly, with whom he had more than one trait of conformity, an extreme modesty pushed almost to timidity, joined to delicate health, prevented Le Royer from making that mark in science to which he might have pretended. The following is a list of the articles which he published jointly with Dr. Prevost:

1. Note on the free acid contained in the stomach of the herbivorê, *(Memoirs of the Society of Physics and Natural History, vol. III, 2d part.)*

2. A memoir on digestion in the ruminants, *(Bibliotheque Universelle for* 1824, *vol. XXVII.)*

3. Observations on the contents of the digestive canal in the fœtus of the vertebrates, *(Bibliotheque Universelle, vol. XXIX.)*

Lastly, he published alone in the *Bibliotheque Universelle, vol. XXVI,* a memoir on the active principle contained in the "purple digitalis."

Having become a valetudinarian in 1850, in consequence of rheumatic affections, Le Royer was struck, in 1860, with cerebral apoplexy, which kept him riveted to his chair till the moment of his death, a few weeks since, without any notable abatement of his intellectual faculties.

APPENDIX ON THE TRICHINIASIS.

We annex the following additional information in respect to Trichiniasis, mentioned in the preceding article:

A few months ago there was a festive celebration in Hettstädt, a small country town near the Hartz Mountains, in Germany. Upwards of a hundred persons set down to an excellent dinner, and having enjoyed themselves *more majorum,* separated, and went to their homes.

Of these one hundred and three persons, mostly men in the prime of life, eighty-three are now in their graves; the majority of the twenty survivors linger with a fearful malady; and a few only walk apparently unscathed among the living, but in hourly fear of an outbreak of the disease which has carried away such numbers of their fellow-diners.

They had all eaten of a poison at that festive board, the virulence of which far surpasses the reported effects of *aqua tophana,* or of the more tangible agents described in toxicological text-books. It was not a poison dug out of the earth, extracted from plants, or prepared in the laboratory of the chemist. It was not a poison administered by design or negligence. But it was a poison unknown to all concerned; and was eaten with the meat in which it was contained, and of which it formed a living constituent.

When the festival at Hettstädt had been finally determined upon, and the dinner had been ordered at the hotel, the keeper of the tavern arranged his bill of fare. The introduction of the third course, it was settled, should consist, as usual in those parts of the country, of *Rostewurst und Gemüse.* The *Rostewurst* was, therefore, ordered at the butcher's the necessary number of days beforehand, in order to allow of its being properly smoked. The butcher, on his part, went expressly to a neighboring proprietor, and bought one of two pigs from the steward, who had been commissioned with the transaction by his master. It appears, however, that the steward, unfortunately, sold the pig which the master had not intended to sell, as he did not deem it sufficiently fat or well-conditioned. Thus the wrong pig was sold, carried on a barrow to the butcher, killed and worked up into sausages. The sausages were duly smoked and delivered at the hotel. There they were fried and served to the guests at the dinner table.

On the day after the festival, several persons who had participated in the dinner were attacked with irritation of the intestines, loss of appetite, great prostration and fever. The number of persons attacked rapidly increased; and great alarm was excited in the first instance by the apprehension of an impending epidemic of typhus fever or continued fever, with which the symptoms observed showed great similarity. But when, in some of the cases treated by the same physician, the features of the illness began to indicate at first, acute peritonitis, then pneumonia of a circumscribed character, next paralysis of the intercostal muscles and the muscles in front of the neck, the hypothesis of septic fever, though sustained in other cases, had to be abandoned with respect to these particular cases. Some unknown poison was now assumed to be at the bottom

of the outbreak; and an active inquiry into all the circumstances of the dinner was instituted. Every article of food and material was subjected to a most rigid examination, without any result in the first instance. But when the symptoms in some of the cases invaded the muscles of the leg, particularly the calves of some of the sufferers, the description which Zenker had given of a fatal case of trichinous disease was remembered. The remnants of sausage, and of pork employed in its manufacture, were examined with the microscope, and found to be literally swarming with encapsuled trichinæ. From the suffering muscles of several of the victims small pieces were excised, and under the microscope found charged with embryonic trichinæ in all stages of development. It could not be doubted any longer, that as many of the one hundred and three as had had partaken of *Rostewurst* had been infested with trichinous disease by eating of trichinous pork, the parasites of which had, at least in part, escaped the effects of smoking and frying.

This awful catastrophe awakened sympathy and fear throughout the whole of Germany. Most of the leading physicians were consulted in the interest of the sufferers, and some visited the neighborhood where most of the afflicted patients remained. But none could bring relief or cure. With an obstinacy unsurpassed by any other infectious or parasitic disease, trichiniasis carried its victims to the grave. Many anthelmintics were arrayed to destroy, if not the worms already in the flesh, at least those yet remaining in the intestinal canal. Picric acid was employed until its use seemed as dangerous as the disease; benzole, which had promised well in experiments upon animals, was tried, but was unavailing. As patient after patient died off, and the dissection of each proved the parasites to have been quite unaffected by the agents employed, the conviction was impressed upon every mind that a man afflicted with flesh-worm is doomed to die the slow death of exhaustion from nervous irritation, fever, and loss of muscular power in parts of the system essential to existence.

But medical science had only just unravelled a mystery; and if it could not save the victims, it was determined at least to turn the occasion to the next best account. The cases were therefore observed with care and chronicled with skill. All the multifarious features of the parasitic disease were registered in such a manner that there can hereafter be no difficulty in the diagnosis of this disorder. A valuable diagnostic feature was repeatedly observed, namely, the appearance of the flesh-worm under the thin mucous membrane on the lower side of the tongue. The natural history of trichina in man was found to be the same as that in animals.

All observations led to the conviction that the trichina encapsuled in the flesh is in the condition of puberty. Brought into the stomach, the calcareous capsule is digested with the flesh, and the trichina is set free. It probably feeds upon the walls of the intestines themselves, for the irritation of the intestines begins before the bringing forth of young trichinæ has taken place. Copulation is immediately effected; and within a few hours, or a short portion of days, from sixty to eighty live embryos leave the female, and begin their own career of destruction.

This consists, in the first instance, in an attempt to pierce the walls of the intestinal canal. Great inflammation of the entire surface ensues, ending not rarely in death of the villous or mucous membrane, or in the formation of masses of pus on its surface. Sometimes there are bloody stools. But these severe symptoms only ensue when much trichinous meat has been eaten; when less has been consumed, pain and uneasiness in the abdomen are produced, accompanied, however, in all instances by wasting fever and prostration. The embryos actually pierce the intestines, and are found free in the effusion, sometimes serous, sometimes purulent, which is always poured out into the abdominal cavity. Thence they again proceed towards the periphery of the body, pierce the peritoneum, causing great irritation, and sometimes peritonitis,

to the extent of gluing the intestines together to a coherent mass. They next proceed to the muscles nearest to the abdomen; arrived at the elementary muscular fibres, which, under the microscope, appear as long cylinders with many transverse striæ, they pierce the membranes, enter the fibres, eat and destroy their striated contents, consume a great part of the granular detritus, moving up and down in the fibres until grown to the size necessary for passing into the quiescent state. They then roll up in spiral or other irregular windings, the bags of the muscular fibres collapse, and only where the trichinæ lie a calcareous matter is deposited, perhaps by the trichinæ themselves, which hardens into perfect capsules round the parasites. A muscular fibre may harbor one or several parasites; but every fibre invaded by a single parasite loses its character entirely, and becomes a bag of detritus from one end to the other.

If it be remembered that one ounce of meat filled with trichinæ may form the stock from which in a few days three millions of worms may be bred, and that these worms will destroy in the course of a few weeks not less than two millions of striated muscular fibres, an idea of the extent of destruction produced by these parasites can be formed. We are not in a position to say to what proportion of the fifty or sixty pounds of muscle required for the performances of the human body these two millions of elementary fibres actually amount. In the muscles nearest to the abdomen the destruction is sometimes so complete that not a fibre free from parasites can be found. This amounts to complete paralysis. But death is not always produced by the paralysis; it is mostly the result of paralysis, peritonitis, and irritative fever combined. No case is known in which trichiniasis, after having declared itself, became arrested. All persons affected have either died, or are in such a state of prostration that their death is very probable.

Most educated people in Germany have, in consequence of the Hettstädt tragedy, adopted the law of Moses, and avoid pork in any form. To some of the large pig-breeders in Westphalia, who keep as many as two thousand pigs, the falling of the price of pork has been a ruinous—at the least a serious—loss. In the dining-rooms of the hotels in the neighborhood of Hettstädt notices are hung up announcing that pork will not be served in any form in these establishments. To counteract this panic, the farmers' club of the Hettstädt district gave a dinner, at which no other meat but pork was eaten. But it has had no appreciable effect. The raw ham and sausages of Germany are doomed to extinction; the smoked and fried sausages must necessarily be avoided. * *

In the south of Germany some people now say that it is the Hungarian pigs which are most frequently affected with trichinæ. This rumor, like the famous pork dinner of the farmers' club, may, however, have been set up with the intention of quieting apprehension about the native pigs. We have already mentioned the accident which befell the crew of a merchant vessel. They shipped a pig at Valparaiso, and killed it a few days before their arrival at Hamburg. Most of the sailors ate of the pork in one form or another. Several were affected with trichinæ and died. Of those whose fate could be inquired into, only one seems to have escaped the parasites. Another outbreak in Saxony has carried away twelve persons. A fourth wholesale poisoning by trichinæ is just reported from Offenbach, the Birmingham of Hesse-Darmstadt. Of upwards of twenty persons infected, three had already died when our correspondent's letter left. Numerous sporadic cases of fever, and epidemics of inscrutable peculiarity, but referred to an anomalous type of fever, are now claimed by medical authors, and with much show of reason, to have been outbreaks of trichiniasis, or flesh-worm disease. Several German physicians experimentalized with a view of finding a cure for this terrible disorder. Professor Eckhardt at Giessen, we are told, has obtained permission to try the disease and supposed remedies upon a murderer under sentence of death. We have not been told whether his reward in case of success is to be a commutation of his capital sentence, but

should hope this to be the case. The experiment, even if it should not have the romantic character indicated, will probably teach some curious details of the life of these parasites. Almost everywhere the commonest rules of cleanliness are disregarded in the rearing of pigs. Yet pigs are naturally clean animals, avoiding, like dogs and cats, all contact with ordure. Though they burrow in the earth, and in summer wallow in the mud, they abhor the heaps of excrements mixed with straw in and upon which they are frequently kept. A due regard to cleanliness will prevent trichinæ in the pig. In wild boars, of which many are eaten in the country round the Hartz mountains, trichina has never been found. Neither has it been met with in sheep, oxen, or horses. Beef is the safest of all descriptions of meat, as no parasites have ever been discovered in it. They have also never been found in the blood, brain, or heart of those animals in whose striated muscles they love to reside.—*British Medical Journal.*

[Lately, the common ground-worm has been found to be infested by trichinæ, one of the probable sources of the infection of swine.]

The interest excited by this case has induced a more careful investigation into the consequences resulting from the imprudent use of hog's flesh, and fatal cases have been recently reported in this country. It had, indeed, been long known among men of science that the trichina was occasionally found incysted in the muscles of man in the United States as well as in other countries, but no case of death resulting from the presence of the worms is known to have been observed till recently. In February, 1864, "an instance of the poisoning of a whole family and the death of one member caused by eating ham" infested with the trichina was observed by Dr. Schnetter in the city of New York.* Dr. L. Krombein has since recorded some cases of a fatal nature noticed by him in the western part of the State. Having been summoned to attend a man and his wife resident in the village of Cheektonaga, he found them afflicted apparently with "acute muscular rheumatism of a somewhat peculiar character," and was sustained in his opinion by the concurrent belief of an associate, Dr. Dingler. He subsequently surmised that the symptoms might indicate trichiniasis; and the patients having soon afterwards died, a microscopical examination by Dr. Krombein, assisted by Dr. Homberger, demonstrated the presence of "trichinæ both in the incysted and free state." "The specimen of human muscle taken from one of these cases after death, and also the sausages eaten of, were examined by Dr. J. R. Lothrop and Professor George Hadley under the microscope, and the trichina found in both in great numbers. In the muscle the parasite was free, in the sausage incysted."† Other members of the family, attacked by the same parasite, were only less unhappy in escaping a fatal end.

[The foregoing accounts, though they indicate an alarming cause of disease, point out a ready means by which the evil may be averted, particularly in the great pork marts of this country, namely, inspection by the microscope. It will probably be found that the disease is exceedingly rare, but the assurance which the inspection would give of this fact would be of sufficient importance to warrant its adoption.—J. H.]

* American Medical Times, February 20, 1864.
† Buffalo Medical and Surgical Journal, June, 1864.

EXPERIMENTAL AND THEORETICAL RESEARCHES

ON

THE FIGURES OF EQUILIBRIUM OF A LIQUID MASS

WITHDRAWN FROM THE ACTION OF GRAVITY, &c.

BY J. PLATEAU, PROFESSOR AT THE UNIVERSITY OF GHENT, ETC.

From the Memoirs of the Royal Academy of Brussels.

INTRODUCTION BY THE SECRETARY OF THE SMITHSONIAN INSTITUTION.

[THE interesting investigations of which we commence in this article to give an account consist of a series of parts originally published in the Transactions of the Brussels Academy. A translation of the first three parts was published in Taylor's Scientific Memoirs; the remainder has been translated for this Institution, and the whole will be published in this and the next volume of the Smithsonian Annual Reports. The author has devised an ingenious method by which a liquid may be withdrawn, as it were, from the influence of gravity, and left free to assume the figure or external form which is produced by the interaction of its own molecules. The experiments described in the first and second parts of the series have excited much interest, and have frequently been presented in popular lectures as precise illustrations of the mode of formation of Saturn's ring, and almost conclusive proofs of the truth of the hypothesis of La Place as to the genesis of the solar system.

It should, however, be observed that the force in operation in the phenomena of the heavenly bodies and that in the experiments of our author are very different, and can only give rise to accidental similarities, and not to identical results. Gravity, which is operative in the first case, is the most feeble of all known attractions, while its sphere of action is indefinitely great. On the other hand, molecular attraction, which is operative in the second case, is exceedingly energetic, while its sphere of action only extends to the nearest contiguous particles, and becomes imperceptible at sensible distances. The great power exhibited by the earth on heavy bodies, near its surface, arises from the combined effect of an immense number of attracting atoms. We know that the attraction of the whole earth gives to a body near its surface a velocity of 32 feet in a second, and by comparing the masses and distances from the centre of the earth, and a globe of the same density and a foot in diameter, we can easily calculate the velocity the latter would give a small body near its surface. The velocity thus determined is less than that of an inch in a year. From this result we may infer that small liquid masses, possessed of a slight degree of vicidity, would never assume the form of a globule under the mere force of gravitation. On the other hand, the great power of molecular attraction is shown by the energy with which water is drawn into wood and other porous substances.

But the difference of the two forces is still more strikingly exhibited by the difference of their spheres of attraction. In the case of gravitation every atom, for example, of the earth attracts every other atom of the whole mass, each conspiring with all the others to produce the spherical form. While under the influence of molecular attraction the atoms of a liquid globule only acts upon the other atoms which are immediately around them; and hence the atoms in the interior of a globule are, as it were, in a neutral condition, attracted equally in every direction. The only atoms, therefore, which are active in producing the globular forms and in giving rise to the phenomena described in this memoir, are those at the surface of the liquid, since these are only attracted on one side, and are, therefore, free to exert their energy towards the mass, and their tendency to bring this into the smallest compass, namely, that of a sphere. According to this view a globule of water may be considered an assemblage of atoms, without attraction, compressed into the spherical form by a contractile film, within which the atoms are enclosed. The amount of contractile force of such a film will depend on the energy of the attraction between the contiguous atoms and the degrees of curvature. To illustrate this, let us suppose a slip of India-rubber to be stretched horizontally between two supports. If to the middle of this we attach a small weight, the slip will sag downwards, and the point to which the weight is attached will descend until there is an equilibrium between the weight and the contractile force. If an additional weight be attached, the descent will be increased until a new equilibrium is attained, and so on, the contractile force will increase with the degree of bending. A similar force is exerted at the free surface of all liquids. If this surface is horizontal, the attraction will be equal in every direction in the horizontal plane; but if at any point we press the surface so as to bend it out of this plane, the contractile force will be called forth, tending to bring the point back into its former position. It is this surface contractile force which causes a small globule of water or mercury, when flattened, to spring back into the spherical form when the compressing force is removed. The more the globule is compressed, or the greater the curvature at the circumference, the greater will be the resistance. Hence, also, the smaller the bubble the greater will be the contractile power of its surfaces, and the more energetically will it assume the spherical form. This is converse of the action of gravity, the tendency of which to produce the globular form will be the greater in proportion to the greater size, and consequently less curvature of the surface.

These remarks will enable the reader to comprehend more definitely the nature of the phenomena exhibited in the following paper.

J. H.]

FIRST SERIES.

1. Liquids, being gifted with an extreme molecular mobility, yield with facility to the action of forces which tend to modify their exterior form. But amongst these forces there is one which predominates so much over the rest that it almost entirely masks their action. This force is gravity; this it is which causes liquids to assume the form of vessels which contain them; and it is this, also, which makes smooth and horizontal the portion of their surface which remains free. We can scarcely recognize, along the contour of this free surface, a slight curve which reveals the action of the combined forces of the attraction of the liquid for itself, and of its adherence for the solid matter of the vessel. It is only by observing very small liquid masses, upon which the relative action of gravity is thus weakened, that we can see the influence of other forces upon the figure of these masses manifested in a very forcible manner. Thus the small drops of liquid, placed upon surfaces which they cannot moisten,

assume a spherical form more or less perfect. Leaving these minute quantities, if we wish to observe liquid masses which have freely taken a certain form, we must quit the earth, or rather consider the terrestrial globe itself and the other planets as having been primitively fluid, and having adapted their exterior form to the combined action of gravitation and centrifugal force. Theory then indicates that these masses ought to take the form of spheroids more or less flattened in the direction of their axis of rotation, and observation confirms these deductions of theory. Observation shows us, also, around Saturn, a body of annular form, and theory finds, in the combined actions of gravity and centrifugal force, means of satisfying the equilibrium of that singular form.

If, however, we could, by some means, withdraw from the action of gravity one of the liquid masses upon which we have to operate, at the same time leaving it free to be acted upon by other forces which might tend to modify its form, and if our process allowed of giving to this mass sufficiently large dimensions, would it not be very curious to see it take a determinate figure, and to see this figure vary in a thousand ways with the forces on which it depends? Now I have succeeded, by an extremely simple means, in submitting to the above conditions a considerable liquid mass.

2. Fat oils are, it is known, less dense than water, and more dense than alcohol. Accordingly, we may make a mixture of water and alcohol having a density precisely equal to that of a given oil—of olive oil, for example. Now, if any quantity of olive oil is introduced into the mixture thus formed, it is evident that the action of gravity upon this mass of oil will be completely annihilated; for, in virtue of the equality of density, the oil will only hold the place of an equal mass of the ambient liquid. On the other hand, the fat oils do not mix with a liquor composed of alcohol and water. The mass of oil must therefore remain suspended and isolated in the midst of the surrounding liquid, and it will be perfectly free to take the exterior form which the forces that may act upon it will give to it.

This being supposed, if the molecular attractions of the oil for itself, those of the alcoholic mixture for itself, and those of this mixture for the oil were identical, there would be no reason that the mass of oil left in the midst of the ambient liquid should take spontaneously one form more than another, since, relatively, to all the forces acting upon it, it would be exactly in the same position as an equal mass of alcoholic mixture whose place it would occupy. But it is evident that this identity between the different attractive forces does not exist, and that the attraction of the oil for itself greatly exceeds the two others. The mass of oil, therefore, ought to obey this excess of its own attractive forces.

We thus come to this conclusion, that our mass of oil may be perfectly assimilated to a liquid mass without weight, suspended freely in space, and submitted to its own proper molecular attractions. Now, it is clear that such a mass must take the spherical form.

Well, experiment confirms all this in a complete manner. The mass of oil, whatever its volume, remains, in fact, suspended in the midst of the alcoholic liquid, and *takes the form of a perfect sphere.*

3. In order to obtain this singular result with facility, it is necessary to take certain precautions, which I will describe.

The first concern the formation of the alcoholic mixture. The density of this mixture necessarily varies with the kind of oil which is used. For the olive oil which I employed, and for the purity of which I cannot vouch, the proper mixture marked twenty-two degrees on the areometer of Beaumé. If, therefore, any one wishes to use olive oil, he may always consider the above value as a first approximation, and, by successive attempts, will bring the liquor at length to the exact point which it ought to reach. To accomplish this, a test tube is filled with the liquor, into which a little oil is afterwards poured by means of a long-necked funnel, which reaches about half way down the test tube. The oil, on reaching

the liquor, forms a globule, to which a diameter of about two centimetres* must be given, and which a little shake will detach from the mouth of the funnel if it does not detach itself. Then, accordingly as this globule falls to the bottom of the liquor or rises to its surface, we conclude that the quantity of alcohol of the mixture is too great or too small; we therefore add to this a little water or alcohol, taking care to stir it well, and recommence the experiment of the test tube. The same operations are repeated until the globule of oil remains suspended in the liquor, without appearing to have a tendency either to fall or rise. The mixture may then be considered as approaching very nearly the desired point. I say very nearly, for the globule of oil of the test tube, being of small dimensions, has more difficulty in moving in the liquor than spheres of a large diameter, and it may seem to be in equilibrium of density with the surrounding liquid, whilst for a larger volume of oil this equilibrium does not exist.

4. When the alcoholic mixture, which I presuppose to be contained in a large glass flask of the ordinary form, has attained this point of approximation, the next thing is to introduce the mass of oil. For this purpose the long-necked funnel which has been mentioned above must be again used, and this must reach to a certain depth in the liquor contained in the flask. Letting the funnel rest on the neck of the latter, we pour the oil slowly. Then, if the alcoholic mixture is by chance exactly in the requisite proportions, the oil forms, at the extremity of the neck of the funnel, a sphere, the volume of which increases gradually in proportion as we add this last liquid. When the sphere has attained the volume we desire, the neck of the funnel is withdrawn with caution; the sphere which adheres to it rises with it toward the surface of the liquor, and the oil which it still contains is added to the preceding. Lastly, when the sphere has nearly reached the surface of the alcoholic mixture, a little shake detaches it from the funnel. Ordinarily, however, the mixture has not so exactly the desired density. We then see, in general, several successive spheres of oil formed, which, detaching themselves one after another from the mouth of the funnel, fall slowly to the bottom of the flask, or rise to the surface of the alcoholic liquor. In this case all these spheres should, in the first place, be united into one, which is easily done by the following means. We introduce into one of them the end of an iron wire. The adherence which the oil contracts with this metal then allows the sphere in question to be easily conducted in the ambient liquid, and to be led to join with a second sphere.† By continuing this treatment, we soon succeed in uniting all. Then, according as the whole sphere shall remain at the bottom or on the surface of the liquor, add cautiously to the latter a certain quantity of water or of alcohol; and, after having corked the flask, we next turn it several times slowly, and so as not to disunite the sphere of oil, until the mixture is well effected, which will take place when we no longer perceive any striæ in the liquor on looking through it at a window. Lastly, the same operation is to be repeated until the sphere of oil is perfectly in equilibrium in the surrounding liquor.

5. If the experiment has been made, as I have supposed, in a flask of the ordinary form, that is to say cylindrical, the mass of oil does not, however, appear exactly spherical; it is widened in the horizontal direction. But this is only an optical illusion, attributable to the form of the flask. The latter, with the liquor which it contains, acts in the manner of a cylindrical lens whose axis

* See table of measures at the end of this volume.

† In order thus to compel two spheres to unite, it does not suffice to put them in contact with one another. They might touch for a long time without mingling into one; one would say that they are enveloped in a resisting pellicule which opposes their union. It is also necessary, therefore, to introduce the extremity of the metallic wire into the second sphere, as if we wished to break the partition which separates the two masses. The union is then effected immediately. I shall revert to these phenomena hereafter.

would be vertical, and enlarges in appearance the horizontal dimensions of the object.

In order entirely to avoid this illusion, we must use a vessel of plane smooth sides, formed of plates of glass set in a metal frame, (§ 8.) We then have, in a complete manner, the curious spectacle of a considerable mass of liquid presenting the form of a perfect sphere, and imitating, in some measure, a planet suspended in space.

Instead, also, of the above vessel, a glass balloon may be used, which is more imple and less expensive. In this case, indeed, the mass of oil only appears in its real figure when it occupies the centre of the balloon; but the apparent distortion is small, as long as the sphere is not moved considerably from this centre. A vessel of this kind is very convenient for most of the experiments which I shall describe in this part of the memoir; but it would not serve for those which I shall have to make known subsequently.

6. Now, having obtained, by means of the process above detailed, a fine sphere of oil well suspended, and presenting, I will suppose, a diameter of six to seven centimetres, we shall observe the following circumstances, which it is important to notice before we proceed further:

In the first place, the equilibrium, previously well established, is soon disturbed of itself. At the end of a few minutes we see the sphere quit its place, and rise with extreme slowness towards the upper part of the ambient liquid. If a little alcohol be then added to restore the equilibrium, on treating the mixture by the process of § 4, this equilibrium is again broken in the same manner at the end of a certain time. In fine, it is only by continuing for some days to maintain it by the successive addition of small quantities of alcohol that we come to obtain a permanent equilibrium, which is then no further disturbed, except by an accidental cause, of which we shall speak in the following paragraph. If the temperature does not fall below 18° centigr., the above phenomena are the only ones observed; but sometimes, if the temperature remains below that limit, and always, if it is below 15°, another effect is manifested, namely, a diminution in the transparency of the oil.

These phenomena are owing to a gradual chemical action which takes place between the oil and the alcoholic mixture. The first of these would be very inconvenient in most of the experiments; but, happily, it may be obviated. This can evidently be effected by employing the two liquids only when they have already exerted upon one another all the action of which they are capable. The oil and the alcoholic mixture which I used are now inert with regard to one another, because, having been employed a great number of times, they have had time to exercise the whole of their mutual action. Besides, it is easy, in a short time, to bring the two liquids to that state of relative neutrality, by agitating them together in order to divide the oil, and thus to accelerate the action, then separating them by a suitable process. This operation requires some precautions, which we shall examine in § 24, in order not to interrupt the course of the memoir by details which are not now indispensable. In all that follows we shall always suppose that two liquids thus prepared are employed.

7. Another cause disturbs the equilibrium between the sphere of oil and the ambient liquid. This is the variations of temperature, which alter the equality of the two densities; and the degree of sensibility of such a system in this respect would hardly be conceived. For example, when the vessel is carried into a room a little warmer or colder than that in which it had been before, the sphere soon falls in the first case and rises in the second. On the mere application of the hands to the outside of the vessel, it will be seen, after a few seconds, that the sphere begins to fall.

We must be continually on our guard against these effects of temperature; otherwise, they disturb the experiments. The following is a recent instance

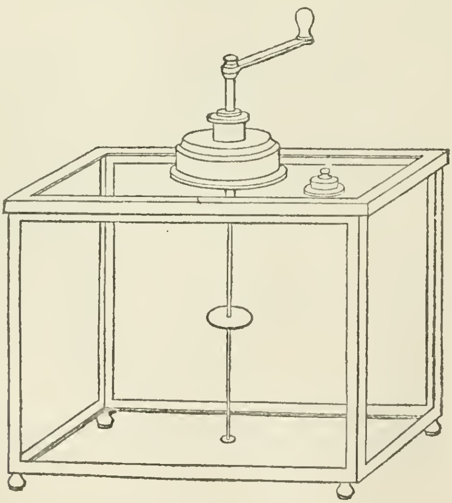
which occurred to me. The oil and the alcoholic liquor were enclosed in different flasks, and the latter contained a very slight excess of alcohol. Having, by chance, carried these two flasks into a room warmer than that in which they had been, I first introduced into the mixture a certain quantity of oil, which, by reason of the slight excess of alcohol, descended slowly to the bottom of the flask. A short time afterwards I poured in another quantity of oil, and I was surprised to see this, on the contrary, rise towards the upper part of the mixture. The reason of the singular difference was this: the alcoholic mixture inclosed in one of the flasks was very considerable in quantity relatively to the oil which the other contained. Now, at the first moment the liquids, not having sensibly changed their temperature, maintained between them the same relation of density; but after a short time the oil, by reason of its small volume, having become warmer than the alcoholic mixture, had thus become relatively lighter. The warmth of the hand which held the flask in pouring out the oil must have also contributed to the effect in question.

8. Now let us suppose a fine sphere of oil in permanent equilibrium in the surrounding liquid, and let us endeavor to submit it to other forces than its own attractions.

The first idea which presents itself is to try the action of centrifugal force. For this purpose it is necessary to impress on the sphere of oil a movement of rotation around one of its diameters, and which is effected by introducing into this sphere a small metallic disc, which is made to turn upon itself by means of an axis which traverses it perpendicularly. This disc carries the oil with it by its adherence, and the whole mass of this liquid takes a movement of rotation.

Before explaining the effects which result from this movement, I shall describe in detail the apparatus I have employed—an apparatus by the aid of which all the experiments succeeded perfectly and with the greatest facility. It is represented in fig. 1.

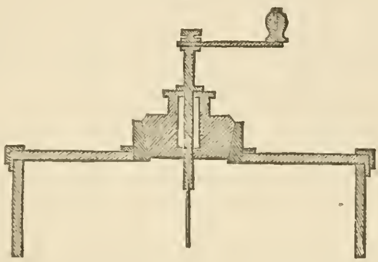
Fig. 1.



The vessel is with plane sides, formed of rectangular plates of glass set in an iron frame; the sides are each 25 centimetres broad and 20 high. The small disc and its axis are also of iron, a metal whose prolonged contact with oil does not stain it as copper does. The diameter of the disc is about 35 millimetres, and the axis is formed of an iron wire about 1½ millimetre thick. This

axis is fixed by its lower end into a hole pierced in the middle of the plate of glass which forms the bottom of the vessel. This hole is closed below by a small plate of iron cemented to the glass. The upper end of the axis is screwed to a larger wire, which forms the prolongation of it, and which, held with a moderate degree of friction, [*à frottement doux,*] in a piece of which I shall speak hereafter, receives at its other extremity the handle by means of which the disc is turned. When the whole system is in place the disc ought to be half way up the vessel. The square plate of glass which closes the vessel above is pieced with two openings, each furnished with an iron neck, which is closed with a stopper of the same metal. One of these openings is in the middle of the plate, and its diameter is 55 millimetres. It is through the stopper which closes it that the rod passes, *à frottement doux,* which receives on the one side the axis of the disc, and on the other the handle. (See figure 2.) The other

Fig. 2.



opening is smaller, and is placed near one of the angles of the plate. It serves for introducing into the vessel either the metallic wire, by the aid of which we unite the partial masses of oil, or additional portions of alcohol, or of mixture at another degree, (§ 9,) &c., when these operations are to be performed without removing the disc from its place. Lastly, this same plate is cemented into an iron frame, which is turned up all round, so as to fit upon a vessel as a lid upon a box. The upper edges of the vessel have been ground with emery all together, after their being placed in the frame, so that the upper plate of glass fits exactly upon them; and by rubbing these edges and the metallic stoppers with a little oil, the vessel, when the plate and stoppers have been placed, may be considered as perfectly closed and keeping the mixture without evaporation of alcohol.

In my apparatus the plates of glass are fixed to the metallic framing by a resinous cement, and this is slightly attacked by the alcoholic mixture. It would perhaps be better to use some glazier's putty; for the alcoholic mixture, being prepared so as not to act any more upon the oil, (§§ 6 and 24,) this latter cement would probably not suffer any alteration. However, the resinous mastic resists to such a degree, that I have been able to leave the alcoholic liquor, without inconvenience, in the vessel for whole months.

The apparatus which I have just described is the best suited for obtaining, in all their beauty, the phenomena, which are the objects of these experiments; but, as I have said above, a hollow sphere of glass of pretty large dimensions might be used with less cost, and without too much disadvantage, at least for the experiments treated of in this part of the memoir. This ought to be furnished with two tubular openings, one of which would serve for introducing the system of the disc, and the other would effect the same object as the second opening of which we have spoken above.

I shall, however, in what follows, suppose all along that the plane-sided vessel above described is the one employed.

9. The apparatus being properly arranged, the next thing is to operate so as to cause a sphere of oil to surround the disc in such manner that their two centres are sensibly coincident. To attain this point, let us first endeavor, before introducing the disc into the vessel, to bring the centre of the sphere to remain at the height at which that of the disc should be. It would be extremely difficult to accomplish this by suspending a sphere in a homogeneous alcoholic mixture, as we have hitherto supposed; for then there is no reason why the sphere should not stand higher or lower; and, if even by chance it were placed exactly at the desired height, the movements which would be produced on introducing the disc would very probably change this height. It is, therefore, necessary to employ a more sure process, and the following succeeded perfectly. We begin by causing the alcoholic mixture to contain a small excess of alcohol. Then, the vessel being furnished with its lid, and the stopper which closes the central opening being lifted up, the mixture is introduced by this opening in such quantity that the vessel be not completely filled. A certain quantity of a mixture, less charged with alcohol, and marking only 16° on the areometer of Beaumé, is then cautiously added. This, from its excess of density, falls to the bottom of the vessel, where it spreads itself in a horizontal layer. The oil is then introduced, which, by reason of the small excess of alcohol contained in the upper mixture, descends through the latter till it rests upon the denser layer of the lower mixture, either in a single mass or in several partial masses (§ 4.) This being so, we unite, if the case requires it, the isolated spheres into a single one; then we stir the liquor cautiously with a glass rod, so as to mix imperfectly the layer at the bottom with the higher layers, but without dividing the mass of oil, and the system is then left to rest. It will be seen that there must hence result in the alcoholic liquor a state of density increasing from the upper layers of less density to the lower of greater density than that of the oil; and that, in consequence, the mass of oil will necessarily remain in stable equilibrium with respect to the vertical direction, in a certain layer whose mean density is equal to its own. Now, in performing the operation with the necessary precautions—that is to say by stirring the liquid only a very little, then leaving it to rest to observe the effect which results, again stirring it and leaving it to rest, and so on; lastly adding, if necessary, a small portion of mixture at 16°, or of pure alcohol, according to circumstances, we easily succeed in causing the mass of oil to remain exactly at the desired height, and, as we have seen, without tendency to a change of position in the vertical direction.* In geometrical strictness, truly, this mass of oil cannot then be any longer perfectly spherical; it must be flattened a little in the vertical direction; but, if we have operated so that the increase of the densities is very feeble at the height at which the oil stands—and we easily obtain that result by suitable trials—the flattening in question is completely insensible to the eye, and the mass appears exactly spherical.

For the experiments which we have to describe, the most convenient diameter to give to the sphere of oil is about 6 centimetres. We easily accomplish this by first forming a less sphere, and adding successively fresh portions of oil, which we unite with the first.

The next thing is to place the disc. This being attached by its axis to the rod which passes through the metallic stopper, (§ 8,) we begin by oiling it as well as the axis, then introduce it slowly into the alcoholic liquid, and cause it to penetrate by its edge into the sphere of oil. As the disc has previously been oiled, the sphere envelopes it without difficulty, and, what is remarkable,

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* The different liquid layers thus superposed tend of themselves, it is true, to mix; but, as they are placed in the order of their densities, this spontaneous mixture proceeds only with extreme slowness, and it requires a great many days for the liquor to become homogeneous. No inconvenience therefore results from this for the experiments

gradually of itself assumes such a position that the axis of the disc traverses it diametrically. This effect is evidently owing to the attractive action of this axis, or rather of the coating of oil with which it has been moistened—an action which tends to operate in a symmetrical manner all around it, and thus brings the entire sphere of oil into a position symmetrical with respect to this same axis. Now it will be seen that the centre of the sphere tending, on the one hand, to remain at the height of that of the disc, on account of the superposition of the alcoholic layers of unequal density, and, on the other hand, to place itself in the axis of the disc, on account of the symmetry of the attractive actions exerted by the latter upon the oil, the centre of the sphere and that of the disc will coincide, and will thus remain in a fixed position. Only the sphere will then be slightly elongated in the vertical direction by the attraction of the axis of the disc; but this elongation is very trifling if the sphere present, as we have supposed, a diameter of 6 centimetres.

10. The sphere of oil being thus suitably placed, we slowly turn the handle. We then presently see the sphere *flatten at its poles and swell out at its equator*, and we thus realize on a small scale an effect which is admitted to have taken place in the planets.

However, although the results may be of the same nature in the case of the great planetary masses and in that of our little masses of oil, I must not omit to remark here that there is an essential difference between the forces which are in play in the two cases. In the first, the force which tends to give to the great planetary mass a spherical figure, and against which the centrifugal force acts, is universal attraction; in the second, the force which acts the same part with regard to the small mass of oil, is molecular attraction, which is subject to different laws. But as, on either hand, the aggregate of the actions reduces itself to a contest between centrifugal force and another force tending to preserve the spherical form of the liquid mass, it appears that the results must be analogous, if not identical, with respect to the figure which that mass assumes.

[This, we do not think, is quite correct. The forces which produce the equilibrium of the ring are as follows: First. The centrifugal force which tends to throw the atoms from the centre of motion. Second. The force developed by the external and internal horizontal curvatures, the direction of which is towards the centre. Third. The force developed by the external and internal vertical curvatures, one of which acts towards the centre, and the other from the centre. The roundness of the ring is caused by the combined action of the external and internal curvatures, which, under no circumstances of velocity of rotation, would produce a flattened ring.—J. H.]

In order to observe, in all its beauty, the phenomenon on which we are engaged, the handle must, at first, be turned with very little velocity—a turn in five or six seconds. The effects are even then very decided. If we afterwards apply a somewhat greater velocity—for example, a turn in four seconds—the flattening at the axis, and the swelling at the equator, are seen to be more considerable, and they are further augmented by increasing the velocity of the handle to one turn in three seconds. Before proceeding further we may remark that, in these experiments, the handle must not be turned too long, for the mass of oil which, in the first moments, presents exactly a figure of revolution, eventually loses this form. At each fresh trial, therefore, the system must be left to repose. The oil then resumes its spherical form, and slowly, of itself, replaces itself in the proper position. The change of form which supervenes when too many turns are given to the disc occasions results of a particular kind, and which are not without interest. I shall speak of them by-and-by, (§ 22.)

11. Now, if instead of moving the handle slowly a considerable velocity is given to it, as two or three turns in a second, new, and very curious, phe-

nomena are manifested. The liquid sphere first takes rapidly its maximum of flattening, then becomes hollow above and below around the axis of rotation, stretching out continually in a horizontal direction, and, finally abandoning the disc, is *transformed into a perfectly regular ring*, (fig. 3.)

Fig. 3.

This ring is rounded transversely, and appears to have a circle for its generatrix. At the moment of its formation its diameter increases rapidly up to a certain limit; when this is reached the movement of the disc must be stopped. The ring now remains for some seconds in the same state. Then, the resistance of the ambient liquid weakening its movement of rotation, it returns upon itself and changes back into a sphere around the disc and its axis.

The velocity of the handle most suitable for producing a beautiful ring, is about three turns per second. The ring thus obtained has a mean diameter of 9 to 10 centimetres.

12. When, at the instant of the formation of the ring, the mass of oil which constitutes it separates from the disc, a singular circumstance is observable; the ring remains united to the disc by an extremely thin pellicle or film of oil, which fills all the space between them. But at the instant that, the ring having reached its greatest extent, we stop the motion of the disc, this pellicle breaks and disappears of itself, and the ring then remains perfectly isolated.

It may be conceived that this pellicle is not a circumstance essential to the phenomenon of the formation of the ring; and we shall see, in another part of these experiments, that it is probably connected with an order of facts wholly different.

13. The heavens exhibit to us also a body of a form analogous to our liquid ring. I allude to Saturn's ring. That, indeed, is flattened, whilst the transverse contour of ours appears altogether round; but I do not think that this difference is so great as it appears at first.

In fact, the centrifugal force, which goes on increasing from the inner circumference of the ring of oil up to its outer circumference, necessarily tends to stretch this ring in the direction of its breadth, or, in other words, to flatten it. But the flattening must be of very small amount; for, on account of the inconsiderable dimensions of the ring, and the slowness of its angular movement, the kind of traction which results from the variation of centrifugal force must be very trifling in comparison with the forces developed by molecular attraction.

14. It appears to me, then, that we may reasonably admit that our ring of oil is in reality slightly flattened, and that in consequence it only differs from that of Saturn, with regard to general form, in the less quantity of flattening.* But further, in the system of Saturn, the flattening of the ring is in part determined by the attraction of the central planet. Now, at the first moment of the formation of the ring of oil, the latter is submitted to a particular force, which plays a part analogous to that of the above attraction. In fact, this attraction acts with the greatest intensity at the inner circumference of Saturn's ring, and thence decreases rapidly in the rest of this body. Now, at the first moment of the formation of the ring of oil, we have seen (§ 12) that the latter remains united to the disc by a thin film of the same liquid, and we may convince ourselves that this film exerts, on the inner circumference of the ring, a considerable force of traction. In fact, if we stop the movement of the disc a little too soon, that is to say a little before the ring has reached its maximum

* I leave out of the question here the subdivision of the ring of Saturn. This subdivision, as is known, is not essentially connected with the conditions of equilibrium of the ring.

of diameter, the film of oil does not break, and the ring then returns upon itself (§ 11) with a much greater rapidity than when the film of oil is broken, and the ring remains isolated. The traction which the film of oil exerts on the inner circumference of the ring ought therefore to produce an effect analogous to that of the attraction of Saturn, that is to say, contribute to increase the flattening. Well, the ring of oil before the rupture of the film presents a very marked flattening. In order to obtain it perfectly, care must be taken that the sphere be well centred in relation to the disc, before beginning the experiment; and it is useful to turn the handle with a velocity somewhat less than that indicated at § 11; the most suitable velocity has appeared to me to be about two turns in a second. As soon as the film of oil breaks the flattening disappears, and the generatrix of the ring becomes, as we have seen, sensibly circular.*

15. Geometricians, who have investigated the figure of equilibrium of a liquid mass in rotation, have only regarded the case in which the attraction which counteracts the centrifugal force is that of universal gravitation, and they have demonstrated that elliptical figures in that case satisfy this equilibrium. Are we thence to conclude that the annular form developed by the rotation of our mass of oil results from the different law which governs molecular attraction, (§ 10,) and that, in the instance of the heavenly bodies, the figure of an isolated ring could not be produced by the sole combination of centrifugal force and of the mutual attractions of the different parts of the mass? I am not of that opinion, and I think it, on the contrary, very probable that if calculation could approach the general solution of this great problem, and lead directly to the determination of all the possible figures of equilibrium, the annular figure would be included among them. This general and direct solution presenting very great difficulties, geometricians have contented themselves with trying whether elliptical figures could satisfy the equilibrium, and with proving that they in fact do satisfy it; but they leave the question in doubt, whether other figures would not fulfil the same conditions. In truth, M. Liouville, in his last researches on this subject,† appears at first view to have nearly solved the question, by introducing the consideration of the stability of the figure of equilibrium, and showing that for each value of the moment of rotation, or, in other words, for any initial movement, whatever, of the mass, there is always an elliptical figure, either of revolution or of three unequal axes, according to

* I had thought that it would be possible to obtain rings isolated and greatly flattened by operating upon larger masses of oil, for then, the ring having a larger volume, the influence of the molecular attraction should be less. But I have found that, in operating on larger masses, it was necessary, in order to obtain the ring in a regular manner, to employ a more feeble velocity of rotation, so that, if the influence of the molecular attraction was diminished, that of the centrifugal force was so equally. The flattening, then, did not become more sensible; or, if I have sometimes imagined that I observed any, I have not been able to reproduce it at will. I have operated thus on spheres which were, successively, about 10, 11, 12, and 14 centimetres in diameter, with discs of a diameter of from seven to nine centimetres, and in a vessel with plane surfaces, having a bottom 35 centimetres square, and a depth of 25 centimetres. The effects, however, thus obtained are very beautiful. The rings are magnificent; present a considerable diameter, and remain sometimes for eight to ten seconds before returning on themselves. With a sphere of ten centimetres diameter, a disc of seven, and a velocity a little less than one turn of the disc per second, we obtain, in a very beautiful and very marked manner, the flattening resulting from the traction of the film of oil.

These experiments, however, are inconvenient and difficult, on account of the large dimensions of the vessel, and the great quantity of alcoholic liquid necessary to fill it.

It may be conceived, moreover, why a larger mass of oil requires a less velocity of rotation to produce a regular ring. It is precisely because the molecular attraction has less influence; whence, it results that, if we attempt to employ the same velocity of rotation which would give a beautiful ring with a less quantity of oil, the mass disunites, and is scattered into spherules.

† The memoir of M. Liouville was communicated to the Academy of Sciences in the sitting of the 13th of February in this year. An analysis of it may be found in the Journal *L'Institut*, No. 477.

the circumstances, which constitutes a form of stable equilibrium. It appears, in effect, natural to admit that, for a given disturbance of a liquid mass, there is but one single final state admissible; and, in this case, this state must necessarily possess stability. However, I do not deem the conclusion which may be drawn from these results so general as it appears at first sight. Without doubt, for a primitive disturbance given, there is only one final state possible, and that state must be stable. But the condition of stability of a found figure of equilibrium does not necessarily involve the consequence that this figure will constitute the final state in question, for it may happen that several figures of equilibrium, corresponding to the same primitive disturbance, might equally possess stability, and that the choice of the mass for one of these figures may have been determined by other circumstances; for example, by the modifications which its movement experiences in the first moments of rotation. In fact, it is by examining these modifications, to which the attention of geometricians has not been directed, that I shall attempt to arrive at the mode of generation of annular figures.

16. When the mass begins to revolve upon itself, the angular velocity of the portions remote from the axis, which are carried off by their centrifugal force, necessarily goes on diminishing. This diminution is especially apparent on the equator of the mass, and it is the more considerable in proportion as the initial movement of rotation was more rapid. It thence results that, in the first instants of a sufficiently rapid rotation, there will be a great difference of angular velocity between the portions which are near the axis and those which are near the equator. Nevertheless, if we admit for a moment that, in virtue of the adherence of the liquid for itself, and of the friction of its several parts, the portions which turn most rapidly communicate by degrees a part of their velocity to the others, so that in the end the result is a mean angular velocity, corresponding to the same moment of rotation, and equal in all the points of the mass, this may take an ellipsoidal figure. But long before the feeble forces, of which we have just spoken, can bring about this mean result, another order of phenomena would be manifested, which may impede the development of the elliptical figure and give rise to an annular form.

In fact, it follows necessarily from the preceding considerations that, in the first instants of a rotation sufficiently rapid, the centrifugal force at the equator of the mass will be much less than that which would correspond to the above mean velocity; and that, on the other hand, the centrifugal force of the portions near the axis will be by much superior to that which would correspond to the same mean velocity. The liquid next the axis will, therefore, be driven towards the liquid of the equator, whence there will necessarily result the formation of a sort of circular cushion, *(bourrelet,)* more or less marked. In other words, the mass will soon become hollow in the middle, and will swell out all around. Now as soon as this phenomenon takes place, it will be conceived that the attraction exerted by this *bourrelet* on the liquid remaining around the axis must be an addition to the action of the centrifugal force, and contribute to increase the volume of the *bourrelet* at the expense of the central liquid. Hence, therefore, it may evidently result that all the liquid will leave the axis for the *bourrelet*, and the latter become in a manner a veritable ring.

This generation of the annular figures would therefore be independent of the law which the attraction follows, and would be, in consequence, the same in the case of universal attraction and in that of molecular attraction.

17. It is easy to verify this mode of generation upon our mass of oil, or at least to assure ourselves that during the formation of the *bourrelet* and of the ring, the angular velocity is much less at the equator of the mass than towards the axis. For this purpose I shall first point out that when a certain number of experiments have been performed upon the same mass of oil, and this has been several times disunited and reformed into a single sphere and into a ring,

it always holds within it a multitude of small bubbles of alcoholic liquor, which borne along by the oil that surrounds them, render the movements of the different points of the mass perfectly observable. Now, if the experiments which we have described be repeated with the aid of a sphere of oil thus filled with alcoholic bubbles, the following results are observed. So long as we give to the disc such slight velocities only as are sufficient to produce a simple flattening, there is not a great difference of angular velocity between the portions next to the axis and the portions adjoining the equator; but this difference becomes very considerable when the disc turns more rapidly, and the *bourrelet* and the ring are developed.

We may thus prove, by means of the small alcoholic bubbles, that the mean angular velocity is established in the ring once formed, and that all the points of the latter perform their revolutions in the same time.

Furthermore, in our experiments upon the masses of oil, there are two foreign forces which act, in addition to the causes which we have noticed, to facilitate the development of the *bourrelet* and of the ring. One is the resistance of the ambient liquid, which contributes to weaken the angular velocity of the equator of the mass; the other is the action of the hand which keeps up the motion of rotation of the disc, and consequently hinders the central portions of the mass from participating gradually in the slackening of the equatorial portions. But that which is produced by these two foreign forces would be equally produced by a greater initial velocity of rotation if we could annul them.

18. When, by the aid of a moderate velocity of the disc, we limit ourselves to producing the flattening of the mass, the two foreign forces of which we have just spoken necessarily hinder the latter from attaining an angular velocity equal in all its points, even though we keep turning the disc. The result is, that the mass cannot take exactly the figure which would correspond to that equality of angular velocity. That which it adopts is a figure of revolution; but on placing the eye at the height of the centre of the mass, it is easily recognized that it is not an ellipsoid. The curvature at the equator is too small, and this is the more evident in proportion as the flattening is more considerable.

Now, is this difference between the figure thus produced and that which would correspond to the case of universal gravitation solely the result of the action of the two foreign forces in question, or is it in part caused by the difference of the laws which the two kinds of attraction follow? In other words, if we could eliminate or render insensible the differences of angular velocity of the several parts of the mass of oil, would the figure produced be an ellipsoid or not? Now, we should render these differences of angular velocity insensible if we could impress a movement of rotation on a mass of oil suspended in an isolated manner, without interior system, in the alcoholic liquid, and then leave it to itself. In this case the resistance of the ambient liquid would be exercised, indeed, on the exterior of the mass; but nothing maintaining the constancy of velocity of the central parts, these, by virtue of the strong self-adherence of the oil, would participate eventually in the slackening of the exterior portions, and we might consider the mass as having each instant an angular velocity equal throughout.

Now, it is very easy to realize the above by availing ourselves of the fact that, when the ring of oil is formed, it returns, after some time, upon itself, (§ 11.) At the instant when the ring is well developed, and when we have just stopped the disc, we lift the latter cautiously by means of the metallic stopper which bears its axis. Then the mass of oil, which is again formed by the return of the ring upon itself, continues still to revolve for some time, completely isolated the ambient liquid. Its figure is then, as well as the eye can judge of it, a perfect ellipsoid of revolution, which gradually approximates to a sphere in

proportion as the rotatory motion becomes weaker.* Thus, the difference of the laws which govern the two sorts of attraction appears not to influence the nature of the figure taken by the mass that turns upon itself.

19. A liquid mass can only assume and preserve an annular form under the influence of a sufficient centrifugal force. Thus, as we have seen, when the resistance of the alcoholic liquid has diminished below a certain limit the velocity of rotation of the ring of oil, the latter, obeying the preponderating action of the molecular attraction, returns upon itself, loses its annular form, and reconstitutes itself into an entire mass, first ellipsoidal and then spherical. But if, by a method which I shall describe, we prevent the ring from agglomerating thus, and still leave the action of its centrifugal force to diminish, we then witness the appearance of other phenomena well meriting interest. In order to produce them perfectly, in place of the disc of 35 millimetres, a disc of about 5 centime

* I had expected to be able to obtain a revolving isolated mass by means of another process, viz: by forming a sphere of oil in the middle of a cylindrical flask so arranged as to be able to turn upon its axis; then causing this flask thus to turn with rapidity, until all the liquid within, alcoholic mixture and mass of oil, had taken the same motion; then suddenly stopping the flask. In effect, it seems that then the alcoholic liquor being the first to lose its rotatory motion by the friction against the stationary sides of the flask, a moment must occur when the mass of oil maintains an excess of angular velocity over the ambient liquid, and that then the effects of centrifugal force upon that mass may manifest themselves. But the experiment gives few results. First, it is extremely difficult to keep a mass of oil in the middle of the flask. We keep it tolerably in the axis of the latter, because, if we have succeeded in placing it so that its centre is little removed from that axis, the rotation of the ambient liquid brings it there, and then retains it there very well. But it is not the same in the direction of the height of the flask. If a homogeneous alcoholic mixture be employed, and the sphere of oil is placed, before turning the flask, a little higher or lower than the middle of the height of the latter, it quits its place when the flask turns to ascend, in the first case, or to descend, in the second, until it comes to be dispersed against one of the two bases of the flask. This effect is attributable, I think, to the fact that the two bases exercising upon the sections of liquid which touch them a motive action much greater than that to which the parallel sections of the interior of the mass are subjected, there ensues near these bases, at the commencement of the rotation, an excess of centrifugal force which determines a tendency upwards and downwards of the liquid near the axis. It is therefore necessary to endeavor to place the sphere of oil in a position very near to the middle of the height of the vessel. Unfortunately we cannot use for this purpose the process of superposition of the alcoholic layers of unequal density, (§ 9;) for then, in the rotation of the flask, the denser inferior layers come necessarily, by the excess of centrifugal force which results from their excess of density, to rise against the sides, causing the less dense liquid to occupy the axis; and in this movement the mass of oil is drawn downwards, and is also dispersed upon the bottom of the vessel.

By employing a homogeneous alcoholic mixture and a sphere of oil of only about three centimetres diameter, I however succeeded several times, by dint of patience, in giving to this sphere a sufficiently exact position in the flask to be able to keep it at the same height until it had itself taken the rotatory movement of the whole system. But then, when I stopped the flask, a violent internal agitation was produced, which almost always dispersed the oil in innumerable spherules throughout the alcoholic liquid, or at least destroyed its form in a completely irregular manner. I attribute these effects to the following cause. When the flask is stopped, the portions of the alcoholic liquid which touch the sides and bases, losing first their centrifugal force, the more internal portions, which still retain theirs, make their way through them, dividing them, and this confusion is soon propagated to the axis, where it gives rise either to the dispersion or to the irregular disfiguring of the mass of oil.

In the cases in which I have been able to give a suitable position to the sphere of oil, I have observed a curious effect; namely, that in the first instance of the rotation of the vessel the mass of oil quits the spherical form, and becomes elongated in the direction of the axis of rotation. This elongation is easy explained: the movement of rotation is communicated to the portions of the mixture which are nearest the axis above and below the mass of oil, before being able to communicate itself with the same intensity to the latter: hence, in the different points of this mass, there must result a less centrifugal force than in the points of the alcoholic mixture situated at the same distances from the axis of rotation. Thence a rush of the oil to the axis, and an elongation of the mass of the latter in the direction of this same axis. But, on continuing the rotation, the oil comes to receive the same movement as the surrounding liquid, and it also resumes gradually the spherical form.

On stopping the flask, not suddenly, but in a rather rapid manner, I succeeded once in obtaining a result sufficiently regular, and I observed, as I expected, the sphere become flattened considerably in the direction of the axis of rotation.

tres was substituted *, which renders necessary, in order to form the ring well, a less velocity of rotation than with the preceding disc, (the most suitable appears to me to be a little less than two turns in a second.) Now, instead of stopping the movement of the disc at the instant when the ring has attained its greatest development, we must continue to move the handle. The film of oil will then break in a little time, as if the disc had been stopped; but, the latter continuing to revolve in the alcoholic liquor, the portions of that liquor which are in contact with it will themselves assume a rotatory movement, and the centrifugal force which results from it will drive them continually towards the ring, so that the latter will not be able to return upon itself. Now, in these circumstances, we soon see the ring lose its regularity, then divide into several isolated masses, each of which immediately takes the spherical form. Thus the ring, when it cannot preserve its figure on account of the decrease of its centrifugal force, and an obstacle prevents its reforming itself into a single sphere, resolved itself into several isolated spheres. As soon as the separation begins to take place, the movement of the disc must be stopped.

This is not all: one or more of these spheres are then almost always seen to assume, at the instant of their formation, a movement of rotation upon themselves—a movement which constantly takes place in the same direction as that of the ring. Moreover, as the ring, at the instant of its rupture, had still a remainder of velocity, the spheres to which it has given birth tend to fly off at a tangent; but as, on the other side, the disc, turning in the alcoholic liquor, has impressed on this a movement of rotation, the spheres are especially carried along by this last movement, and revolve for some time around the disc. Those which revolve at the same time upon themselves consequently then present the curious spectacle of planets revolving at the same time on themselves and in their orbit. The movement of rotation of these masses is, however, too slow, in relation to their diameter, to cause any sensible flattening. Finally, another very curious effect is also manifested in these circumstances. Besides three or four large spheres into which the ring resolves itself, there are almost always produced one or two very small ones, which may thus be compared to satellites.

The experiment which we have just described, presents, as we see, an image in miniature of the formation of the planets, according to the hypothesis of Laplace, by the rupture of the cosmical rings attributable to the condensation of the solar atmosphere.

20. When some oil is introduced into a mixture containing a little excess of alcohol, a phenomenon is observable, which is connected with that of the resolution of the ring into isolated spheres. If the oil be poured in with sufficient rapidity it forms a long cylindrical train, extending from the beak of the funnel to the bottom of the vessel, where the mass gathers. Now, this kind of tail, which connects the mass of oil with the beak of the funnel, remains as long as the oil which forms it has a sufficiently rapid movement of translation—that is to say, as long as we continue to pour; but, as soon as we cease to pour out, and the movement of translation is slackened, the train of oil is instantly resolved into several isolated spherules.

21. The formation of a ring analogous to that of Saturn naturally inspires the desire to carry further the resemblance to the system of that planet, and to seek, whether, by some modification of our experiment, it would not be possible to contrive so that a sphere of oil should remain in the middle of the ring. Now, I have succeeded in producing this effect, by means of a process which I shall proceed to describe; only that this experiment must be regarded merely as a scientific sport, for the circumstances which give rise to the result have evidently no analogy with those which can have occasioned the configuration of the system of Saturn.

* This substitution is accomplished by detaching the upper end of the axis of the first disc from the large wire which passes through the metallic stopper (§ 8,) and screwing in its place the end of the axis of the new disc.

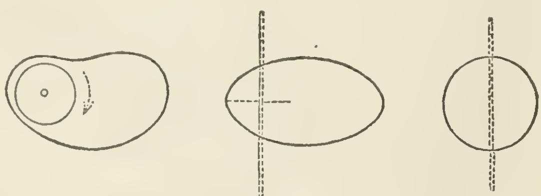
It is first necessary to be able to give to the disc a considerable velocity of rotation. To do this, we adapt to the upper part of the vessel a system of two pulleys—one small, and fixed on to the prolongation of the axis of the disc at the place of the handle, which is taken away; the other larger, and to the axis of which the same handle is attached. In my apparatus the diameters of the two pulleys are, respectively, 12 and 75 millimetres. In the second place, the diameter of the sphere being always nearly six centimetres, that of the disc should be only two centimetres. Lastly, the disc should not have, as in the preceeding experiments, its centre coinciding with that of the sphere. It should be placed lower, toward the inferior part of the latter.

Matters being thus arranged, the handle is turned with a velocity which experience soon enables us to determine. In my apparatus this velocity ought to be about two turns and a half per second, which nearly corresponds to fifteen turns of the disc in the same time. We then see, in general, a ring rapidly formed, which extends itself, leaving in its centre a mass of oil, to which it remains united by a thin pellicle. At the instant when the ring has attained a sufficient development, (and by habit alone can this be correctly learned,) the rotation is suddenly stopped. The pellicle then breaks, the ring remains completely isolated, and the central mass forms into a sphere. We have thus, during some instants, a curious representation of the system of Saturn, except the flattening of the ring. The ring returns rapidly, afterwards, upon itself, and is again united to the central sphere. This experiment does not offer any great difficulties. It requires, however, some skill to succeed perfectly.*

22. In describing (§ 10) the experiment in which the flattening of the sphere is effected by the immediate action of the disc, I have remarked that the movement of the latter should not be continued too long, because the mass of oil then comes to lose its form. Now, if we continue, nevertheless, to turn the handle, with a view to observe the results of this disfigurement, we see manifested new and very capricious effects.

The sphere being well centred with relation to the disc, if we give velocities of one turn in six, five or four seconds to the latter, we begin, after seven or eight turns, to see the mass of oil elongate itself horizontally in one direction, taking a form which resembles much an ellipsoid of three axes; and, what is more singular, this kind of ellipsoid is placed in an eccentric manner with relation to the axis of rotation. Figure 4 represents, for a velocity of a turn in

Fig. 4.



four seconds, the mass viewed from three different sides; that is to say, from above and in the two lateral directions of the smallest and of the largest horizontal axis: the dotted parts indicate the positions of the disc and of the axis of rotation. The aspect of the mass seen from above shows that it is slightly bent in one direction; but this effect is evidently owing to the resistance of the ambient liquid.

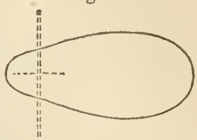
When once the mass has taken this form, it preserves it indefinitely as long as the movement of the disc continues; it continues to revolve eccentrically

* On communicating this very experiment to the academy, in the sitting of April, 1842, (see the Bulletins,) I stated that it was necessary to vary the velocity of rotation. I have since found that, having adopted a convenient velocity, it was best to keep it uniform.

round the latter, and with a velocity much less than that of this disc. This inferior velocity, I may add, evidently also proceeds from the resistance of the ambient liquid.

If a greater velocity is given to the disc without, however, passing a certain limit—if, for example, we give it one turn in three seconds, the phenomena are still of the same kind; only the mass is more elongated, the flexure due to the resistance of the ambient liquid is more decided, and the form is more removed from an ellipsoid. Figure 5 represents the mass viewed on the side, and showing to the eye its greatest length.

Fig. 5.



If the velocity of the disc is increased to a turn in two seconds, the phenomena become less constant and less regular. We should say that there is, for this velocity, a transition from one order of phenomena to another, and that the mass hesitates between the two.

In fact, with a velocity still a little greater, namely, about one turn in a second and a half, the phenomena begin again to be regular and constant, but they are different from the first. They are exhibited in all their beauty when the velocity is increased to a turn in a second. The mass then is at first deeply hollowed around the axis, as if the ring was on the point of being developed; and it remains under this form of a circular *bourrelet* during sixteen to eighteen turns of the disc; we then see it elongate gradually according to a horizontal diameter, but no longer eccentrically, so that, seen from above, it presents an elliptic figure sometimes very perfect, of which the disc occupies the centre, (fig. 6.) This ellipse then lengthens more and more, rather rapidly, and begins to bend

Fig. 6.

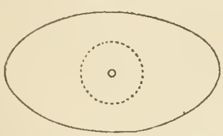
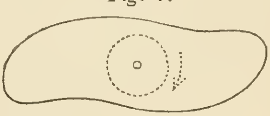
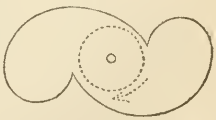


Fig. 7.



by the resistance of the ambient liquid, (fig. 7.) Lastly, on a sudden the mass becomes strongly inflected from both sides, and its form seen from above is then as represented in fig. 8. The mass afterwards preserves this last form in a perfectly fixed manner, as long as the movement of the disc continues.

Fig. 8.



23. However capricious these phenomena may appear, chance, or accidental causes, have still no part in them. I have repeated a great number of times the experiments detailed above, and the effects have aways been identically the same for the same velocities.

After having seen the stable figures which the mass takes in these circumstances, we cannot help making a comparison between these figures and the ellipsoids of three axes of MM. Jacobi and Liouville, (§15,)—ellipsoids which are also always, as the latter of these geometricians has shown, figures of stable equilibrium. Would the identity of the phenomena in the case of universal gravitation and in that of molecular attraction hold good so far? Doubtless the singular figures which we have just described are not ellipsoids; but their aspect admits of our attributing the difference to the resistance of the ambient liquid, which on one side determines the flexures of which we have spoken, and on the other maintains a permanent inequality of angular velocity between the portions adjoining the disc and the more distant portions. Calculation alone could inform us up to what point the above comparison is well founded; the complete solution of the problem, for the case of molecular attraction, would perhaps not present difficulties so insurmountable as for that of universal attraction.

24. In all the experiments which I have described in this memoir, I have supposed that the oil and the alcoholic mixture were rendered chemically inert with regard to each other, and I have said (§ 6) that it was easy in a short space of time to obtain two such liquids. I proceed now to detail the process by means of which this object is attained.

We begin by making a mixture of alcohol and distilled water, containing a certain excess of alcohol, so that when submitted to the trial of the test tube (§ 3) it lets the small sphere of oil fall to the bottom rather rapidly. After having formed the mixture in quantity more than sufficient to fill the vessel which is to serve for the experiments, we introduce into this same mixture a quantity of oil about double what is considered necessary for these experiments.* If a flask is not at hand large enough to contain the whole, we divide the masses among several separate flasks: but care must then be taken that each one may contain the same proportions of water, alcohol, and oil. After this we invert these flasks rapidly a great number of times, but without shaking them, until the oil has been divided into spherules of the size of a pin's head; the whole is then left to rest. Then if the alcohol of the mixture is in proper quantity, the spherules should sink with extreme slowness, so as to take about a quarter of an hour for the greater part to collect at the bottom of the flasks. If it is otherwise, water or alcohol is to be added, as may be required; the contents to be mixed by inverting the flasks several times, as above, then left again to settle, and the operation thus to be recommenced until the result is obtained which I have described. When this point is obtained the whole is thrown upon filters, care being taken to cover the funnels containing these last with plates of glass. This precaution is necessary in order to prevent, as much as possible, the evaporation of the alcohol, and for another reason, of which we shall speak hereafter. The alcoholic liquor passes the first through the filters, ordinarily carrying with it a certain number of very minute spherules of oil. When the greater part has thus passed, the spherules become more numerous. What still remains in the first filters, namely, the oil, and a residue of alcoholic liquor, is then thrown into a single filter placed on a new flask. This last filtration takes place much more slowly than the first, on account of the viscosity of the oil. It is considerably accelerated by renewing the filter once or twice during the operation. If the funnel has been covered with sufficient care the oil will collect into a single mass at the bottom of the flask, under a layer of alcoholic liquor.

The preceding operations have thus given us the following results: On the one hand, the inert alcoholic mixture, still holding a small excess of alcohol, and containing a certain number of small spherules of oil; on the other hand, the oil equally inert, and covered with a little of this same alcoholic liquid. Now, a second filtration completely clears the first from the spherules which it holds. With respect to the oil, it is extracted from below the alcoholic layer by means of a small siphon, armed with a lateral tube, and received into a dry flask, which is to be perfectly corked. In this manner we have the two liquids separate and inactive, with regard to each other. When it is desired to use them, if we perceive that the alcoholic liquid is a little too dense, we correct it with pure alcohol; and if, on the contrary, there is too little density, we correct it with alcohol at 16 degrees. In this latter case we must not use pure water, because this, when it mixes with the prepared alcoholic liquor, produces in it a cloudiness more or less decided.

The various trials I have made relatively to the above process, have led me to ascertain that the two liquids, when they have not been submitted to this preparation, are both modified by their mutual contact. The alcoholic liquid

* It is indispensable to have the two liquids thus in excess, on account of the quantities which are necessarily lost during the different operations which we shall describe, and in the preparation of the experiments.

dissolves some oil, and this in its turn probably dissolves some alcohol. It is especially from the modification which the oil undergoes that its great diminution of relative density results. (§ 6.) Now, when the oil thus modified remains exposed to the air, it passes again gradually to the state of fresh oil, and resumes its former density. It is partly to avoid this that I have recommended the funnels which enclose the filters to be kept constantly covered, and the oil to be kept in a flask perfectly corked. As for the alcoholic mixture, it is evident that this last precaution is equally necessary.

25. Before I conclude, I must forewarn those persons who may wish to repeat my experiments of two effects which sometimes occur, and which cause disturbance in the operations if the experimenter does not know the means of preventing or destroying them.

When some oil is introduced into a mixture containing an excess of alcohol, it happens sometimes that the mass which has sunk to the bottom of the vessel contracts adherence with this bottom and spreads itself out more or less on its surface. There is then no means of removing it entire; but the spreading of the adhesion may be prevented by contriving that the bottom of the vessel should be occupied by a layer of a mixture more dense than the oil, (§ 9.)

The second effect to which I allude is presented in the inverse case—that is to say, when the sphere of oil, instead of reaching the bottom of the vessel, rises, on the contrary, to the surface of the alcoholic liquor, either because this liquor contains too little alcohol, or on account of a lowering of temperature, or because we have not been able to use prepared oil. When this happens the mass flattens at first, more or less, at the surface of the mixture, as if this last opposed a resistance to it. Then, after some time, it makes its way through, and then presents a portion of plane surface, more or less extended, on the level with that of the alcoholic liquor. But what occasions trouble is, that then, so to speak, it has contracted an adherence with this same surface, from which it is not detached without great difficulty. It is, at first, easy to prevent the production of this effect by pouring on the surface of the liquor a small layer of pure alcohol; and this same means will serve also to destroy the effect in question, if it is already produced. In this latter case we may again invert the vessel with caution. The movement thus imparted to the ambient liquor suffices, ordinarily, to detach the mass of oil, with the exception of a small portion, which almost always remains adhering to the surface.

26. Lastly, I have already mentioned the fact that, after a certain number of experiments, the oil becomes filled with small spherules of alcoholic liquor. Now, reciprocally, the ambient alcoholic liquor is also often sprinkled with a multitude of small spherules of oil. It is scarcely necessary to remark that, when all these spherules have become too numerous, and we desire to restore the liquids to their original transparency, this is easily accomplished by filtrations similar to those of which I have spoken above, (§ 24.)

27. We have been hitherto engaged with the figures assumed by a liquid mass abstracted from the action of gravity and submitted to the attraction of its molecules, either when this mass is at rest, or when a movement of rotation upon itself is imparted to it. Notwithstanding the difference of the laws which the attractive forces follow in this case and in that of the large planetary masses, we have seen produced, on a small scale, a striking representation of the majority of the phenomena of configuration relative to the celestial bodies. In the second part of this investigation we shall submit our liquid masses to new forces, and we shall then see developed a series of phenomena quite as curious but of a different class.

NOTE.—Professor Faraday, who has repeated many of M. Plateau's remarkable and beautiful experiments, coloured his oil green, for the purpose of rendering it more distinctly visible in the spirit, by dissolving in it a little oxide of copper. This, he states, is easily done by heating a little oil with the oxide, and then mingling that with the rest.

SECOND SERIES.

PREFACE.

At the period when attacked by the disease which has entirely deprived me of sight, I had terminated the greater part of the experiments relating to this series, as well as the following. M. Duprez, correspondent of the Brussels Academy, and M. Donny, had the kindness to undertake those which were still wanting. I constantly directed their execution; nearly all were made in my presence, and I followed all the details. I have therefore considered myself justified, in order to simplify the description, in expressing myself in the course of this investigation as if I had made the experiments.

With respect to the theoretical portions, I am indebted to the able assistance of one of my colleagues, M. Lamarle, who has most kindly devoted many long hours to listening to the details of my investigations, and to aiding me in the explanation of several difficult points. I am also indebted to another of my colleagues, M. Manderlier, for the execution of a part of the calculations.

May I be permitted to express in this place my gratitude to these devoted friends? Thanks to their generous help, science is still an open field for me: notwithstanding the infirmity with which I am afflicted, I am able to put in order the materials I have collected, and even to undertake fresh researches.

Preliminary considerations and theoretical principles. General condition to be satisfied by the free surface of a liquid mass withdrawn from the action of gravity, and in a state of equilibrium. Liquid sphere.

1. The process described in the previous memoir enabled us to destroy the action of gravity upon a liquid mass of considerable volume, leaving the mass completely at liberty to assume the figure assigned to it by the other forces to which it is subject. This process consists essentially in introducing a mass of oil into a mixture of water and alcohol, the density of which is exactly equal to that of the oil employed. The mass then remains suspended in the surrounding liquid, and behaves as if withdrawn from gravity. By this means we have studied a series of phenomena of configuration, dependent either simply upon the proper molecular attraction of the mass, or upon the combination of this force with the centrifugal force. We shall now abandon the latter force, and introduce another of a different kind, the molecular attraction exerted between liquids and solids; in other words, we shall cause the liquid mass to adhere to solid systems, and study the various forms assumed under these circumstances by those portions of the surface which remain free. In this way we shall have the curious spectacle presented by the figures of equilibrium appertaining to a liquid mass, absolutely devoid of gravity and adherent to a given solid system.

But the figures which we shall obtain present another kind of interest. The free portions of their surface belong, as we shall show, to more extended figures, which may be conceived by the imagination, and which, in the same condition of total absence of gravity, would belong to a perfectly free liquid mass; thus our processes will partially realize the figures of equilibrium of a mass of this kind. The latter are far from being confined to the sphere; but among them the sphere alone is capable of being completely formed, the others presenting either infinite dimensions in certain directions, or other peculiarities which we shall point out, and which equally render their realization in the complete state impossible.

Moreover, the results at which we shall arrive will constitute so many new and unexpected confirmations of the theory of the pressures exerted by liquids upon themselves in virtue of the mutual attraction of their molecules, a theory upon which the explanation of the phenomena of capillarity is based.

Lastly, in our liquid figures we shall discover remarkable properties, which will lead us to some important applications.

2. In order to guide us in our experiments, and also to enable us to comprehend their bearing, we shall first consider the question in a purely theoretic point of view. The action of gravity being eliminated and the liquid mass being at rest, the only forces upon which the figure of equilibrium will depend will be the molecular attraction of the liquid for itself, and that exerted between the liquid and the solid system to which we cause it to adhere. The action of the latter force ceases at an excessively minute distance from the solid; hence, in regard to any point of the surface of the liquid situated at a sensible distance from the solid, we have only to consider the first of the two above forces, *i. e.*, the molecular attraction of the liquid for itself.

The general effect of the adhesive force exerted between the liquid and the solid is to oblige the surface of the former to pass certain lines; for instance, if a liquid mass of suitable volume be caused to adhere to an elliptic plate, the surface of the mass will pass the elliptic outline of the plate. At every point of this surface, situated at a sensible distance from this margin, the molecular attraction of the liquid for itself alone is in action.

Let us now examine into the fundamental condition which all points of the free surface of the mass must satisfy, in virtue of the latter force.

The determination of this condition and its analytical expression are comprised in the beautiful theories upon which the explanation of the phenomena of capillarity is based, although geometricians have not specially studied the problem of the figure of a liquid mass void of gravity adherent to a given solid system. We shall, therefore, now resume the principles and the results of the theories in question, at least those which relate directly to our subject.

3. Within the interior of a liquid mass, at any notable distance from its surface, each molecule is equally attracted in every direction; but this is not the case at or very near the surface. In fact, let us consider a molecule situated at a distance from the surface less than the radius of the sphere of sensible activity of the molecular attraction, and let us imagine this molecule to be the centre of a small sphere having this same radius. It is evident that one portion of this sphere being outside the liquid, the central molecule is no longer equally attracted in every direction, and that a preponderating attraction is directed towards the interior of the mass. If we now imagine a rectilinear canal, the diameter of which is very minute, to exist in the liquid, commencing at some point of the surface in a direction perpendicular to the latter, and extending to a depth equal to the above radius of activity, the molecules contained in this minute canal, in accordance with what we have stated, will be attracted towards the interior of the mass, and the sum of all these actions will constitute a pressure in the same direction. Now, the intensity of this pressure depends upon the curves of the surface at that point at which the minute canal commences. In fact, let us first suppose the surface to be concave, and let us pass a tangent plane through the point in question. All the molecules situated externally to this plane, and which are sufficiently near the minute canal for the latter to penetrate within their sphere of activity, will evidently attract the line of molecules which it contains from the interior towards the exterior of the mass. If, therefore, we suppressed that portion of the liquid situated externally to the plane, the pressure exerted by the line would be augmented. Hence it follows that the pressure corresponding to a concave surface is less than that which corresponds to a plane surface, and we may conceive that it will be less in proportion as the concavity is more marked.

If the surface is convex, the pressure is, on the contrary, greater than when the surface is plane. To render this evident, let us again draw a tangent plane at that point at which the line of molecules commences, and let us imagine for a moment that the space included between the convex surface and this plane is filled with liquid. Let us then consider a molecule, m, of this space sufficiently near, and from this point let fall a perpendicular upon the minute canal. The

action of the molecule m upon the portion of the line comprised between the base of the perpendicular and the surface will attract this portion towards the interior of the mass. If afterwards we take a portion of the line equal to the former from the other side of the perpendicular, and commencing at the base of the latter, the action of the molecule m upon this second portion will be equal and opposite to that which it exerted upon the first; so that these two portions conjointly would neither be attracted towards the interior nor the exterior of the mass; if beyond these two same portions another part of the line is comprised within the sphere of activity of m, this part will evidently be attracted towards the exterior. The definitive action of m upon the line will then be in the latter direction. Hence it follows that all the molecules of the space comprised between the surface and the tangent plane which are sufficiently near the line to exert an effective action upon it, will attract it towards the exterior of the mass. If, then, we suppress this portion of the liquid so as to reproduce the convex surface, the result will be an augmentation of the pressure on the part of the line. Thus the pressure corresponding to a convex surface is greater than that corresponding to a plane surface, and its amount will evidently be greater in proportion as the convexity is more marked.

4. If the surface has a spherical curvature, it may be demonstrated that, representing the pressure corresponding to a plane surface by P, the radius of the sphere to which the surface belongs by r, and by A a constant, the pressure exerted by a line of molecules, and reduced to unity of the surface, will have the following value:

$$P+\frac{A}{r}, \qquad (1.)$$

r being positive in the case of a convex, and negative in that of a concave surface.

Whatever be the form of the surface, let us imagine two spheres, the radii of which are those of greatest and least curvature at the point under consideration. It is evident that the pressure exerted by the line will be intermediate between those corresponding to these two spheres, and calculation shows that it is exactly their mean. Denoting the two radii in question by R and R′, the pressure exerted by the line, referred to the unity of surface, would be

$$P+\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right) \qquad (2.)$$

The radii R and R′ are positive when they belong to convex curves, or, in other terms, when they are directed to the interior of the mass; whilst they are negative when they belong to concave curves, *i. e.*, when they are directed towards the exterior.

5. From the preceding details we can now easily deduce the condition of equilibrium relative to the free surface of the mass.

The pressures exerted by the lines of molecules which commence at the different points of the surface are transmitted to the whole mass; consequently, for the existence of equilibrium in the latter, all the pressures must be equal to each other. In fact, let us imagine a minute canal running perpendicularly from some point of the surface, and subsequently becoming recurved so as to terminate perpendicularly at a second point of this same surface, it is evident that equilibrium can only exist in this minute canal when the pressures exerted by the lines which occupy its two extremities are equal; and if this equality exists, equilibrium will necessarily exist also. Now, the pressures exerted by the different lines depend upon the curves of the surface at the point at which they commence; these curves must therefore be such, at the various points of the free surface of the mass, as to determine everywhere the same pressure.

Such is the condition which it was our object to arrive at, and to which in each case the free surface of the mass must be subject.

The analytical expression of this condition is directly deducible from the general value of the pressure given in the preceding paragraph; we only require to equalize this value to a constant, and, as the quantities P and A are themselves constant, it is in fact sufficient to make

$$\frac{1}{R}+\frac{1}{R'}=C, \quad\text{.........................}\quad (3.)$$

the quantity C being constant for the same figure of equilibrium.

This equation is the same as those which are given by geometricians for capillary surfaces, when, in the latter equations, the quantity representing gravity is supposed to be O.

R and R′ may be replaced by their analytical values; we are thus led to a complicated differential equation, which only appears susceptible of integration in particular cases. Yet the equation (3) will be useful to us in the above simple form. Now we know that the normal plane sections which correspond to the greatest and the least curvature at the same point of any surface form a right angle with each other. Geometricians have shown, moreover, that if any two other rectangular planes be made to pass through the same normal, the radii of curvature, ρ and ρ', corresponding to the two sections thus determined, will be such that the quantity $\frac{1}{\rho}+\frac{1}{\rho'}$ will be equal to the quantity $\frac{1}{R}+\frac{1}{R'}$. Hence the first of these two quantities may be substituted for the second; and, consequently, the equation of equilibrium, in its most general expression, will be

$$\frac{1}{\rho}+\frac{1}{\rho'}=C, \quad\text{.........................}\quad (4.)$$

in which equation ρ and ρ' denote the radii of curvature of any two rectangular sections passing through the same normal.

6. These geometric properties lead to another signification of the equation (4.) We know that unity divided by the radius of curvature corresponding to any point of a curve is the measure of the curvature at this point. The quantity $\frac{1}{\rho}+\frac{1}{\rho'}$ represents, then, the sum of the curvatures of two normal rectangular sections at the point of the surface under consideration. This being admitted, if we imagine that the system of the two planes occupies successively different positions in turning around the same normal, a sum of curvatures $\frac{1}{\rho}+\frac{1}{\rho'}, \frac{1}{\rho''}+\frac{1}{\rho'''}, \frac{1}{\rho^{\text{iv}}}+\frac{1}{\rho^{\text{v}}}$, &c., will correspond to each of these positions; and, according to the property noticed in the preceding paragraph, all these sums will have the same value. Consequently, if we add them together, and let n denote the number of positions of the system of the two planes, the total sum will be equal to n times the value of one of the partial sums, or to $n\left(\frac{1}{\rho}+\frac{1}{\rho'}\right)$. Now, this total sum is that of all the curvatures $\frac{1}{\rho}, \frac{1}{\rho'}, \frac{1}{\rho''}, \frac{1}{\rho'''}$, &c., in number $2n$, corresponding to all the sections determined by the two planes. If, then, we divide the above equivalent quantity by $2n$, the result $\frac{1}{2}\left(\frac{1}{\rho}+\frac{1}{\rho'}\right)$ will represent the mean of all these curvatures. Now, as this result is independent of the value of n, or of the number of positions occupied by the system of the two planes, it will be equally true if we suppose

this number to be infinitely great, or, in other words, if the successive positions of the system of the two planes are infinitely approximated, and consequently if this same system turns around the normal in such a manner as to determine all the curvatures which belong to the surface around the point in question. The quantity $\frac{1}{2}\left(\frac{1}{\rho}+\frac{1}{\rho'}\right)$ represents, then, the mean of all the curvatures of the surface at the same point, or the mean curvature at this point. Now if, in passing from one point of the surface to another, the quantity $\frac{1}{\rho}+\frac{1}{\rho'}$ retains the same value, *i. e.*, if for the whole surface we have $\frac{1}{\rho}+\frac{1}{\rho'}=C$, this surface is such that its mean curvature is constant.

Considered in this purely mathematical point of view, the equation (4) has formed the object of the researches of several geometricians, and we shall profit by these researches in the subsequent parts of this memoir.

Thus our liquid surfaces should satisfy this condition, that the mean curve must be the same everywhere. We can understand that if this occurs, the mean effect of the curvatures at each point upon the pressure corresponding to this point also remains the same, and that this gives rise to equilibrium. Hence we now see more clearly the nature of the surfaces we shall have to consider, and why they constitute surfaces of equilibrium.

6*. We must now call attention to an immediate consequence of the theoretical principles which have led us to the general condition of equilibrium. According to these principles, each of the lines of molecules exerting upon the mass the pressures upon which its form depends, commences at the surface and terminates at a depth equal to the radius of the sensible activity of the molecular attraction, so that these lines collectively constitute a superficial layer, the thickness of which is equal to the radius itself, and we know that this is of extreme minuteness. It results from this that the formative forces exerted by the liquid upon itself emanate solely from an excessively thin superficial layer. We shall denominate this consequence *the principle of the superficial layer.*

7. A spherical surface evidently satisfies the condition of equilibrium, because all the curvatures in it are the same at each point; also when our mass is perfectly free, *i. e.*, when it is not adherent to any solid which obliges its surface to assume some other curve, it in fact takes the form of the sphere, as shown in the preceding memoir.

8. Before proceeding further, we ought to elucidate one point of great importance in regard to the experimental part of our investigations. The liquid mass in our experiments being immersed in another liquid, the question may be asked whether the molecular actions exerted by the latter exert no influence upon the figure produced; or, in other words, whether the figure of equilibrium of a liquid mass adherent to a solid system, and withdrawn from the action of gravity by its immersion in another liquid of the same density as itself, is exactly the same as if the mass adherent to the solid system were really deprived of gravity and were placed *in vacuo*. Now, we shall show that this really is the case. The molecular actions resulting from the presence of the surrounding liquid are of two kinds, viz., those resulting from the attraction of this liquid for itself, and those resulting from the mutual attraction of the two liquids. Let us first consider the former, imagining for an instant that the others do not exist. The surrounding liquid being applied to the free surface of the immersed mass, the former presents *in intaglio* the same figure as the latter mass presents in relief. Those molecules of this same liquid which are near the common surface of the two media must then exert pressures of the same

nature as those which we have considered throughout the preceding details, towards the interior of the liquid to which they belong, and these pressures must consequently also impart a figure of equilibrium to the surface *in intaglio;* so that if the immersed mass of itself had no tendency to assume any one figure rather than another, the surrounding liquid would give it a determinate one, by compelling it to mould itself in the above hollow figure. This is why a bubble of air in a liquid assumes the globular form, solely in consequence of the pressures exerted by the liquid upon it. Now let us suppose that the immersed mass has assumed that figure which it would acquire *in vacuo* if really deprived of gravity; the analytical condition of paragraph 5 would then be satisfied as regards this mass. Now at each point of the common surface of the two media, the radii of curvature ρ and ρ' have the same absolute values, both in the case of the immersed mass and of the hollow figure of the surrounding liquid, except that their signs are contrary, according as they are considered as referring to one or the other of the two liquids. To pass from one of the two figures to the other, we need therefore only change the signs ρ and ρ', or, what comes to the same thing, change the sign of the constant C. Changing the sign does not destroy the condition of equilibrium; and consequently, if the immersed mass is in equilibrium as regards its own molecular attractions, the same will hold good in the case of the hollow figure of the surrounding liquid. The pressures of the latter liquid cannot, therefore, by themselves produce any modification in the figure of equilibrium of the immersed mass.

Let us now introduce the second kind of molecular actions, *i. e.*, the mutual attraction of the two liquids, and see what will be its effects. Let us imagine, for an instant, that the immersed mass, or, for the sake of fixing the ideas, the mass of oil in our experiments is replaced by the same kind of liquid as that which surrounds it, *i. e.*, by the alcoholic mixture. In other words, supposing the vessel to contain only the alcoholic mixture and the solid system, let us limit, in the imagination, a portion within the liquid of the same figure and dimensions, and situated in the same manner as the preceding mass of oil. It is then clear that the molecules of the mass near its surface being, like those of the interior, completely surrounded by the same kind of liquid beyond their sphere of activity, these molecules will no longer exert any pressure upon the mass; consequently, the pressures which would exist if this mass could be isolated must be considered as destroyed by the attractions emanating from the surrounding liquid. The latter forces are, therefore, all equal and opposite to the pressures in question. Now, as these are all equal to each other in accordance with the figure which we have attributed to the imaginary surface of the mass, the attractions emanating from the surrounding liquid will also all be equal to each other. If we now replace the mass of oil, the attractions emanating from the surrounding liquid may certainly alter in absolute value, but it is evident that they will retain their directions, and that they will remain equal to each other. We therefore see that they will only diminish, by the same quantity, all the pressures exerted by the mass of oil upon itself; consequently, as all the differences remain equal to each other, the condition of equilibrium will still be satisfied as regards that mass. It is evident that the same mode of reasoning may be applied to the pressures exerted by the surrounding liquid upon itself—pressures which will retain their directions, all of which will only be diminished to the same extent by the attractions emanating from the oil, so that the condition of equilibrium will still be satisfied as regards the hollow figure of the surrounding liquid. Thus the whole of the molecular actions due to the presence of the surrounding liquid will not tend in any way to modify the figure of equilibrium of the immersed mass, which figure will, consequently, be identically the same as if that mass were really void of gravity and were placed *in vacuo.* We can, therefore, leave the surrounding liquid completely

out of the question, its sole function being to neutralize the action of gravity upon the mass forming the object of the experiments.

9. We shall now pass to the experimental part. And first, to avoid useless repetition, we shall say a few words relative to the apparatus to be used. As the liquid always consists of a mass of oil immersed in an alcoholic mixture of the same density as itself, our solid systems will all consist of iron, and this for the following reasons: In ordinary circumstances oil contracts, I believe, perfect adhesion with all solids; but this is not exactly the case when the same oil is plunged into a mixture of water and alcohol; for then, in the case of certain solids, as, *e. g.*, glass, the phenomena of adhesion sometimes undergo modifications which give rise to trouble in the experiments. We shall meet with an instance of this in the subsequent parts of this memoir. Now, the metals do not present this inconvenience; moreover, the form which we have given to most of our solid systems would render their construction of any other substance besides a metal difficult. Now, among metals we prefer iron, not copper, because oil removes nothing from iron, whilst by prolonged contact with copper it slightly attacks it, acquires a green color, and increases in density, which is a great inconvenience.*

When we wish to use one of these solid systems of iron, before introducing it into the vessel, it must be completely moistened with oil; and for this purpose it is not sufficient simply to immerse it in this liquid, but it must be carefully rubbed with the finger. The presence of this coating facilitates the adherence of the liquid mass.

We shall continue to make use of the vessel with plane walls, described in the preceding memoir, § 8;† a common-shaped bottle, and the flask previously mentioned (§§ 5 and 8) in the same memoir, are not well adapted, because they do not exhibit the true figure of the mass.

When the solid system is composed of a single piece, it is supported by a vertical iron wire, which is screwed to the lower end of the axis traversing the metallic stopper; but for certain experiments the solid system is formed of two isolated parts, and then only one of them is attached to the axis, as I have stated; the other is supported by small feet which rest upon the bottom of the vessel. It need not be mentioned that those liquids only which are prepared in such a manner as to be incapable of exerting any chemical action upon each other can be employed, (§§ 6 and 24 of the preceding memoir.)

In addition to the little funnel for introducing the mass of oil into the vessel, the iron wire which serves for uniting the isolated spheres, &c., of which I have spoken in the preceding memoir, the experiments require some other accessory instruments, as, in the first place, a small glass syringe, the point of which is elongated and slightly bent. It is used as a sucking-pump, to remove, for in-

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* In a letter which Dr. Faraday did me the honor of sending to me, regarding the preceding memoir, he informed me that, when about to repeat my experiments before a numerous audience, wishing to produce a still greater difference in the aspect of the two liquids, he dissolved intentionally a little oxide of copper in the oil, so as to render the latter of a green color. The compound having thus been made beforehand, and rendered perfectly homogeneous, and the alcoholic mixture having been regulated according to the density of the modified oil, the presence of the copper in solution could not produce any inconvenience; but in this case also the solid systems should unquestionably be made of iron.

† In making the experiments relating to the present memoir, I found that it was requisite slightly to modify the apparatus in question. The second perforation in the plate forming the lid of the vessel should be but little smaller than the central aperture; its neck should be less elevated; and, lastly, it should be placed near the other; if left as previously described and figured, the employment of the accessory instruments which we shall describe would be impossible. Moreover, the neck of the central aperture should be furnished with a slight rim, so that it may be easily taken hold of when we wish to remove the lid, as, *e. g.*, when it is required to attach a solid system which is too large to pass through this same aperture to the axis which traverses the stopper. Lastly, the vessel should be furnished with a stop cock at its lower part, so that it may be easily emptied.

stance, a portion of the oil composing the liquid mass, when it is required to diminish the volume of the latter, or to withdraw the entire mass of oil from the vessel, an operation which is sometimes required, &c. In the second place, two wooden spatulas, one being slightly bent, the other straight, covered with fine linen or cotton stuff. When these spatulas are introduced into the vessel, and the cloth with which they are furnished is thoroughly impregnated with the alcoholic liquid, the mass of oil does not adhere to them. Hence, by means of one or the other of these spatulas, the mass can be moved in the surrounding liquid, and conducted to the place which it is required to occupy in the interior of the vessel without any of it remaining upon the spatula. This is the purpose for which these instruments are intended. After they have been used, care must always be taken to agitate them in pure alcohol before allowing them to dry. If this precaution be omitted, the alcoholic mixture with which they are impregnated, on evaporating, would leave the small quantity of oil which it held in solution upon their surface; and when the same instruments are used again, the mass of oil would adhere to it. In the third place, an iron spatula, the uses of which we shall point out in the proper place. Lastly, as it is necessary, in all the experiments which we shall relate, that the alcoholic liquid should be homogeneous, the process indicated in the preceding memoir (§ 25) cannot be used to prevent the mass of oil from becoming occasionally adherent to the bottom of the vessel; but the same result is obtained by covering the bottom with a square piece of linen.

New experiments in support of the theoretical principles brought forward in the preceding observations. Figures of equilibrium terminated by surfaces of spherical curvature. New principle relating to layers of liquids.

10. The facts which we shall first describe may be considered as constituting the experimental demonstration of the principle of the superficial layer, (§ 6, *bis.*) Let us imagine any solid system to be immersed in the liquid mass, and let us give to this mass such a volume that it may constitute a sphere which completely envelops the solid system without the latter reaching the surface at any point. Then, if the above principle be true, the presence of the solid system will exert no influence upon the figure of equilibrium, because, under these circumstances, the superficial layer, from which the configuring actions emanate, remains perfectly free; whilst if these actions emanated from all points of the mass, any unsymmetrical modification occurring in the internal parts of the latter would necessarily produce one in the external form. This is confirmed by experiment. The condition of a solid system completely enveloped by the mass of oil would be somewhat difficult to realize; but it must be remembered that, in the experiments relating to the preceding memoir, the system of the disk, by means of which the mass was made to revolve, was very nearly in this condition, because it did not reach the external surface of the mass excepting at the two very small spaces which gave passage to its axis. But we then saw (§ 9 of the preceding memoir) that when the mass was at rest, its sphericity was only very slightly altered by the presence of this system. The theoretical condition may be more nearly approached by taking a very fine metallic wire for the axis of this same system; in this case the alteration in form is quite imperceptible. The axis being supposed to be vertical, the disk may, moreover, be placed so that its centre coincides with that of the mass of oil, or is situated above or below the latter without producing any difference. I shall relate another fact of an analogous nature. In the course of the experiments, it sometimes happens that portions of the alcoholic liquid become imprisoned in the interior of the mass of oil, forming so many isolated spheres. Now, however these spheres may be situated in the interior of the mass, not the least alteration is produced in the figure of the latter.

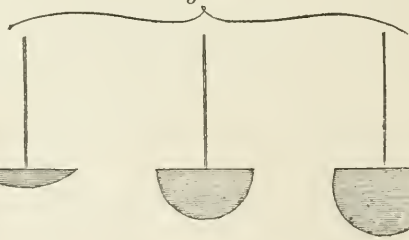
11. Again, let us cause some kind of solid system to penetrate the liquid mass; but now let the mass be of too small a volume to be capable of completely enveloping this system. The latter will then necessarily reach the superficial layer; and, if the principle in question be true, the figure of the liquid mass will be modified, or, in other words, will cease to remain spherical. This does really occur, as we might have expected; the liquid mass becomes extended at those portions of the solid system which project externally from its surface; it finally either occupies the whole of these portions, or only a part of their extent, according to the form and the dimensions of the solid system, and thus assumes a new figure of equilibrium. We shall meet with examples of this hereafter, (§§ 14, 15, 17.)

12. Instead of causing the solid system to penetrate the interior of the liquid mass, let it simply be placed in contact with the external surface of the latter. An action being then established at a point of the superficial layer, equilibrium must be destroyed, and the figure of the liquid mass ought again to be modified. This really occurs; the mass becomes extended upon the surface presented to it, and consequently acquires a different shape. This result might also have been anticipated from what occurs under ordinary circumstances, when a drop of water is placed upon a previously moistened solid surface. One might be induced to believe that, as regards the actual result, this case is referable to that of the preceding paragraph or that in paragraph 10; for it appears that the liquid mass, becoming extended upon the solid system so as to obtain the new figure of equilibrium, should ultimately occupy or envelop this system in the same manner as if the latter had been made to penetrate its interior directly. Under certain circumstances this must occur; but the experiments which are about to be related will show that under other circumstances the result is totally different.

13. Let us take for the solid system a thin circular plate,* attached by its centre to the iron wire which supports it, (Fig. 1,) and let us produce the

Fig 1.

Fig. 2.



adhesion of its lower surface to the upper part of the mass of oil.† Directly contact is completely established, the oil extends rapidly over the surface pre-

* The diameter of that which I have used is 4 centimetres. I mention this diameter for the sake of being definite. It is evident that in our experiments the dimensions of the apparatus are completely arbitrary, except that if these dimensions exceed certain limits, the operations will become embarrassing in consequence of the large quantities of liquid which would be required.

† In order that this operation may be effected with facility, the sphere of oil must first remain in the surrounding liquid beneath the central aperture in the lid; the plate being then introduced into the vessel, we have merely to lower it by means of the axis traversing the stopper to bring it towards the liquid mass. If the latter does not occupy the position in question, it must be previously placed there by means of a spatula covered with linen, (§ 9.) It must be remarked here, that true contact between the plate and the sphere of oil does not usually ensue immediately; a certain resistance has to be overcome, analogous to that treated of in the note to paragraph 4 of the preceding memoir; but to overcome this, the liquid sphere need only be gently moved by means of the plate. The slight resulting pressure soon causes the rupture of the obstacle and the production of adhesion.

sented to it; but, what is remarkable, although the precaution has been taken of rubbing the whole of the system, (§ 9,) that is, the two faces of the plate as well as its rim, with oil, the oil terminates abruptly at this rim without passing to the other side of the plate, and thus presents a sudden interruption in the curvature of its surface. In the case in question, the new figure acquired by the mass is a portion of a sphere. This portion will be as much larger in proportion to the complete sphere as the volume of oil is greater; but the curvature will always terminate abruptly at the margin of the plate. (See Fig. 2, which represents a section of the solid system and the adherent mass in the case of three different volumes of the latter.)

The cause of this singular interruption of continuity is readily understood. The rim of the plate reaching to the superficial layer, it is natural that something peculiar should occur along this margin, and that the continuity of form should cease at that point where a foreign attractive action is exerted without transition on the superficial layer.

14. Let us again make use of the above plate; but instead of presenting one of its faces to the exterior of the sphere of oil, let us insert the plate edgewise into the interior of this sphere.* The liquid will necessarily extend over both faces of the solid; and if the diameter of the primitive sphere were less than that of the plate, the oil will be seen to form two spherical segments upon the two faces in question, the curvatures of which will still terminate abruptly at the margin of the plate. These two segments may be either equal or unequal, according as the edge of the plate has been introduced into the liquid sphere in such a manner that the plane of the plate passes through the centre of the sphere or not. The upper segment will be slightly deformed by the action of the suspending wire; but this effect will be less sensible in proportion to the thinness of the wire in question. Fig. 3 represents the result of the experiment with two unequal segments. The discontinuity of the curvatures is a very general fact, which we shall frequently find to recur in the course of our experiments; it will hereafter lead us to very important consequences.

Fig. 3.

Fig. 4.

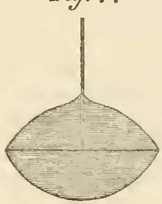


Fig. 5.

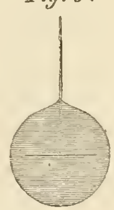
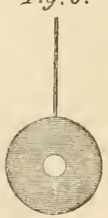


Fig. 6.



15. I have repeated the same experiment, substituting a plate of an elliptic form for the circular plate. In this, as in the preceding case, the oil extends over both faces of the solid, so as entirely to cover them; and, if the volume of

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* This operation is performed as follows: The stopper to which the system of the plate is attached is kept at some distance above the neck of the central aperture, in such a manner, however, that the latter is immersed to a sufficient depth in the alcoholic mixture. The plate can then be moved with tolerable freedom, and it is conducted towards the liquid mass For this purpose the latter ought previously to occupy a suitable position. Immediately the liquid mass is cut, the plate is kept still until the action is terminated, after which the stopper is carefully placed in the neck. A process the reverse to the preceding may also be made use of. The liquid mass is first made to occupy a position near the second aperture, and a sufficient distance from the axis which passes through the centre of the central aperture; then, having fixed the solid system firmly in the position which it is to occupy, move the liquid mass towards it, and when this has been cut, allow the action to continue uninterruptedly. These processes are also employed in other experiments, and it is enough to have pointed them out once. In some cases the second is the only practicable one. This may be easily decided upon in making the experiments.

the liquid mass is not too great, the curvatures again terminate abruptly along the rim of the plate. By gradually augmenting the volume of the primitive sphere of oil, without, however, rendering it sufficiently large to allow of the mass completely enveloping the plate so as to retain the spherical form, a limit is attained at which the edge of the plate ceases to reach the superficial layer of the new figure of equilibrium except at the two summits of the ellipse. The discontinuity in the curvatures then only occurs at these two places. Figs. 4 and 5 exhibit the result of the experiment in this case. In Fig. 4 the long axis of the ellipse is presented to view, in Fig. 5 its short axis.

16. All the facts which we have hitherto detailed show that so long as the interior of the mass is modified its external shape undergoes no alteration; but that directly the superficial layer is acted upon, the mass acquires a different form. To complete the proof, by experiment alone, that the configuring actions exerted by the liquid upon itself emanate solely from the superficial layer, the only point would then be the possibility of reducing a liquid mass to its superficial layer, or at least to a thin pellicle, and to see if in this state it would assume the same figure of equilibrium as a complete mass. Now this is completely realized in soap-bubbles; for these bubbles, when detached from the tube in which they have been made, assume, as is well known, a spherical form, *i. e.*, the same figure as that which we find a complete mass acquires in our apparatus when withdrawn from the action of gravity and perfectly free. When the mass adheres to a solid system, which modifies its figure, it is clear that the entire configurative action is composed of two parts, one of which belongs to the solid system; and we find that this system only exerts it when acting upon the superficial layer; the other belongs to the liquid, and emanates directly from the free portion of this same superficial layer. The facts which we have related show clearly what is the seat of this second part of the whole configurative action, but they do not make us acquainted with the nature of the forces of which it consists. On referring to theory, we find that these forces consist in pressures exerted upon the mass by all the elements of the superficial layer, pressures the intensity of which depend upon the curvatures of the surface at the points to which they correspond. Hence it follows that the mass is pressed upon by every part of its superficial layer, with an intensity depending in the same manner upon the curvatures of the surface. For instance, a mass the free surface of which presents a convex spherical curvature, will be pressed upon by the whole of the superficial layer belonging to this free surface, with a greater intensity than if this surface had been plane; and this intensity will be more considerable in proportion as the curvature is greater, or as the radius of the sphere to which the surface belongs is less. Let us see whether experiment will lead us to the same conclusions.

17. The solid system which we shall employ is a circular perforated plate, (Fig. 6.) It is placed vertically, and attached by a point of its circumference to the iron wire which supports it. Let the diameter of the sphere of oil be less than that of the plate, and let the latter be made to penetrate the mass by its edge in a direction which does not pass through the centre of the sphere. At first, as in the experiment at paragraph 14, the oil will form two unequal spherical segments; but matters do not remain in this state. The most convex segment is seen to diminish gradually in volume, consequently in curvature, whilst the other increases, until they have both become exactly equal. One part of the oil then passes through the aperture in the plate, so as to be transferred from one of the segments towards the other, until the above equality is attained.

Let us now examine into the consequences deducible from this experiment, judging from the preceding ones, and independently of all theoretical considerations. When the oil has once become extended over both surfaces of the plate, in such a manner that the superficial layer is applied to every part of the

margin of the latter, the action of the solid system is completed; and the movements which subsequently ensue in the liquid mass, to attain the figure of equilibrium, can only then be due to an action emanating from the free part of the superficial layer. It is, therefore, the latter which compels the liquid to pass through the aperture in the plate; and the phenomenon must necessarily result either from a pressure exerted by that portion of the superficial layer which belongs to the most convex segment, or by a traction produced by the portion of this same layer belonging to the other segment. Our experiment not being alone capable of determining our choice between these two methods of explaining the effect in question, let us provisionally adopt the first, *i. e.*, that which attributes it to pressure. In our experiment, this pressure emanates from the superficial layer of the most curved segment; but it is easy to see that the superficial layer of the other segment also exerts a pressure which, alone, is less than the preceding. In fact, if for the most curved segment a segment less curved than the other were substituted, the oil would then be driven in the opposite direction. Hence it follows that the entire superficial layer of the mass exerts a pressure upon the liquid which it encloses, and that the intensity of this pressure depends upon the curvatures of the free surface. Moreover, as the liquid proceeds from the most curved segment to that which is least so, it is evident that in the case of a convex surface, the curvature of which is spherical, the pressure is greater in proportion as the curvature is more marked, or as the radius of the sphere to which the surface belongs is smaller. Lastly, since a plane surface may be considered as belonging to a sphere, the radius of which is infinitely great, it is evident that the pressure corresponding to a convex surface, the curvature of which is spherical, is superior to that which would correspond to a plane surface. All these results were announced by theory. They perfectly verify, then, that part of the latter to which they refer, and this concordance ought now to decide in favor of the hypothesis of pressure. This same part of the theory was already verified, in its application to liquids submitted to the action of gravity, by the phenomenon of the depression presented by liquids in capillary tubes, the walls of which they do not moisten; but the series of our experiments, setting out with the elements of the theory, and following it step by step, yields far more direct and complete verification. Our last experiment leads us to still further consequences. The liquid passing from one of its segments to the other, so long as their curvatures have not become identical, and the pressures corresponding to the two portions of the superficial layer becoming equal to each other simultaneously with the two curvatures, it follows that the mass only attains its figure of equilibrium when this equality of pressure is established. We thus have a primary verification of the general theory of equilibrium which governs our liquid figures, a condition in virtue of which the pressures exerted by the superficial layer ought to be everywhere the same. Moreover, it is evident that if a superficial layer, having a spherical curvature, exerts by itself a pressure, this principle must be true, however small the extent of this layer may be supposed to be. It follows, therefore, that an extremely minute portion of the superficial layer of our mass, taken from any part of either of the two segments, ought itself to be the seat of a slight pressure; consequently, that the total pressure exerted by the superficial layer is the result of individual pressures emanating from all the elements of this layer. This was also shown by theory. Further, following the same train of reasoning, we see that the intensity of each of the minute individual pressures ought to depend upon the curvature of the corresponding element of the layer, which is also in conformity with theory. Lastly, as in a state of equilibrium the two segments belong to spheres of equal radii, the curvature is the same in all points of the surface of the mass; whence it follows that all the minute elementary pressures are equal to each other. The general condition of equilibrium (§ 5) is, therefore, perfectly verified in the instance of our experiment.

18. The principle of the superficial layer, applied to the preceding experiment, allows of the latter being modified in such a manner as to obtain a very remarkable result. When the figure of equilibrium is once attained, the perforated plate acts upon the superficial layer by its external border only. The whole of the remainder of this plate then exerts no influence upon the figure in question. Hence it follows that this figure would still be the same if the aperture were enlarged, only the greater the diameter of the latter the less time is required for the establishment of the equality between the two curvatures. Lastly, we ought to be able to enlarge the aperture nearly to the margin of the plate without changing the figure of equilibrium; or, in other words, to reduce the solid system to a simple ring of thin iron wire. Now, this is confirmed by experiment; but, to put it in execution, we cannot confine ourselves, as before, to making the solid system penetrate a sphere of oil of less diameter than that of this same system, and subsequently to allow the molecular forces to act, because the metallic wire, on account of its small extent of surface, would not exert a sufficient action upon the superficial layer to cause the liquid to extend so as to adhere to the entire surface of the ring. The mass would then remain traversed by part of the latter, and its spherical form would not be sensibly altered if the metallic wire were small; the liquid surface would merely be slightly raised upon the wire in the two small spaces at which it issued from the mass. To speak more exactly, under the circumstances in question two figures of equilibrium are possible. One of these differs but very slightly from the sphere; it is not symmetrical with regard to the ring, one part of which traverses it whilst the other part remains free. The second figure is perfectly symmetrical as regards the ring, and completely embraces its margin; its surface is composed of two equal spherical curves, the margins of which rest upon the ring; in other words, it constitutes a true doubly convex lens of equal curvatures. This is the figure which it is our object to obtain. For this purpose we first give the sphere of oil a diameter slightly greater than that of the metallic ring; we then introduce the latter into the mass so that it is completely enveloped; lastly, by means of the small glass syringe, (§ 9,) some of the liquid is gradually removed from the mass.* As this diminishes in volume, its surface is soon applied to every part of the margin of the ring, and the volume continuing to diminish, the lenticular form becomes manifest. Afterwards, by withdrawing more of the liquid, the curvatures of the two surfaces may be reduced to that degree which is considered suitable. In this way a beautiful double convex lens is obtained, which is entirely liquid except at its circumference. Moreover, in consequence of the index of refraction of the olive oil being much greater than that of the alcoholic mixture, the lens in question possesses all the properties of converging lenses; thus, it magnifies objects seen through it, and this magnifying power may be varied at pleasure by removing some of the liquid from, or adding more to, the mass. Our figure, therefore, realizes that which could not be obtained with glass lenses, *i. e.*, it forms a lens, the curvature and magnifying power of which are variable. The diameter of that which I formed was 7 centimetres, and the thickness of the metallic wire was about ½ a millimetre. A much finer wire might have been used with the same success; but the apparatus would then become inconvenient on account of the facility with which it would be put out of shape. By operating with care, the curvatures of the lens may be diminished so as almost to make them vanish; thus I have been enabled to reduce the lens which I formed, and the diameter of which, as I have stated, was 7 centimetres, to such an extent that it was only 2 or 3 millimetres in thickness. Hence we might presume that it would be possible to obtain, by a proper mode of pro-

* The point of the instrument is introduced into the vessel through the second aperture in the lid.

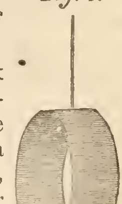
ceeding, a layer of oil with plane faces. This is, in fact, confirmed by experience, as we shall see further on.

19. To render the curvatures of the liquid lens very slight, the point of the syringe must naturally be applied to the middle of the lens, because the maximum of thickness exists there. Now, when a certain limit has been attained, the mass suddenly becomes divided at that point, and a curious phenomenon is produced. The liquid rapidly retires in every direction towards the metallic circumference, and forms a beautiful liquid ring along the latter; but this ring does not last for more than one or two seconds, after which it spontaneously resolves itself into several small, almost spherical masses, adhering to various parts of the ring of iron wire, which passes through them like the beads of a necklace.

20. The reasoning which led us, at the commencement of paragraph 18, to reduce the primitive solid system to a simple metallic wire representing the line in the direction of which this system is met by the superficial layer belonging to the new figure of equilibrium, may be generalized. We may conclude that whenever a solid system introduced into the mass is not met by the superficial layer of the figure produced, excepting in the direction of small lines only, simple iron wires, representing the lines in question, may be substituted for the solid system employed. But if the volume of the primitive solid system were considerable, it would evidently be requisite to add to the mass of oil an equivalent volume of this liquid, to occupy the place of the solid parts suppressed.

There is, however, an exception to this principle; it occurs when the solid system separates the entire mass into isolated portions, as in the experiment of paragraph 14; for then these portions assume figures independent of each other, and which may correspond to different pressures. In this case the suppression of one portion of the solid system would place the figures primitively isolated in communication, and the inequality of the pressures would necessarily induce a change in the whole figure. Excluding this exception, the principle is general, and the result of it is that well-developed effects of configuration may be obtained on employing simple iron wires instead of solid systems. The experiment of the biconvex lens furnishes one instance of this, and we shall meet with a great many others hereafter. Nevertheless, to be enabled to comprehend the influence of a simple metallic wire upon the configuration of the liquid mass, it is not requisite to consider this wire as substituted for a complete solid system; it may also be considered by itself. It is, in fact, clear that the solid wire acting by attraction upon the superficial layer of the mass, the curvatures of the two portions of the surface resting upon it ought not to have any further relation of continuity with each other. The metallic wire may, therefore, determine a sudden transition between these two portions of the surface, the curvatures of which will terminate abruptly at the limit which it places to them. The principles which we have established ought undoubtedly to be considered as among the most remarkable and curious consequences of the principle of the superficial layer, and one cannot avoid being astonished when we see the liquid maintained in such different forms by an action exerted upon the extremely minute parts of the superficial layer of the mass.

Fig. 7.



21. We have experimentally studied the influence of convex surfaces of spherical curvature; let us now ascertain what experiment is able to teach us in regard to plane surfaces and concave surfaces of spherical curvature. Let us take for the solid system a large strip of iron, curved circularly so as to form a hollow cylinder, and attached to the suspending iron wire by some point on its outer surface, (Fig. 7.) To prevent the production of accessory phenomena in the experiment, we shall suppose that the breadth of the metallic band is less than the diameter of the cylinder formed by the same band, or that it is at least equal to it. Make the mass of oil adhere to the internal surface of this

system, and let us suppose that the liquid is in sufficient quantity then to project outside the cylinder. In this case the mass will present on each side a convex surface of spherical curvature, and the curvatures of these two surfaces will be equal. This figure is a consequence of what we have previously seen; and we must not stop here, for it will serve us as a starting point in obtaining other figures which we require. Apply the point of the syringe to one of the above convex surfaces, and gradually withdraw some of the liquid; the curvatures of the two surfaces will then gradually diminish, and with care they may be rendered perfectly plane. It follows from this first result that a plane surface is also a surface of equilibrium, which is evidently in conformity with theory. Let us now apply the end of the syringe to one of these plane surfaces, and again remove a small quantity of liquid. The two surfaces will then become simultaneously hollow, and will form two concave surfaces of spherical curvature, the margins of which rest upon the metallic band, and the curvatures of which are the same. Finally, by the further removal of the liquid, the curvatures of the two surfaces become greater and greater, always remaining equal to each other.

Hence it results, first, that concave surfaces of spherical curvature are still surfaces of equilibrium, which is also in accordance with theory. Moreover, as the plane surface left free sinks spontaneously as soon as that to which the instrument is applied becomes concave, it must be concluded that the superficial layer belonging to the former exerts a pressure which is counterbalanced by an equal force emanating from the opposite superficial plane layer, but which ceases to be so, and which drives away the liquid as soon as this opposite layer commences to become concave. Again, as further abstraction of the liquid determines a new rupture of equilibrium, so that the concave surface opposite to that upon which we directly act exhibits a new spontaneous depression when the curvature of the other surface increases, it follows that the concave superficial layer belonging to the former still exerted a pressure, which at first was neutralized by an equal pressure arising from the other concave layer, but which becomes preponderant, and again drives away the liquid, when the curvature of this other layer is increased.

Hence it follows, first, that a plane surface produces a pressure upon the liquid; second, that a concave surface of spherical curvature also produces a pressure; third, that the latter is inferior to that corresponding to a plane surface; fourth, that it is less in proportion as the concavity is greater, or that the radius of the sphere to which the surface belongs is smaller. These results were also pointed out by theory, and had already been verified in the application of the latter to liquids submitted to the action of gravity, by the phenomenon of the elevation of a liquid column in a capillary tube, the walls of which are moistened by it.

Reasoning upon these facts, as we have done at the end of paragraph 17 in regard to convex surfaces of spherical curvature, we shall arrive at the conclusion that the entire pressure exerted by a concave superficial layer of spherical curvature is the result of minute individual pressures arising from all the elements of this layer, and that the intensity of each of these minute pressures depends upon the curvature of that element of the layer from which it emanates. Our last experiment, therefore, perfectly verifies that part of the theory which relates to plane and convex surfaces of spherical curvature. Lastly, in the state of equilibrium of our liquid figure, the curvature being the same at all points of each of the two concave surfaces, it is again evident that all the minute elementary pressures are equal to each other, which gives a new complete verification of the general condition of equilibrium.

22. The figure we have just obtained constitutes a biconcave lens of equal curvatures, and possesses all the properties of diverging lenses, *i. e.*, it diminishes objects seen through it, &c. Moreover, as the curvature of the two surfaces may be increased or diminished by as small degrees as is wished, it follows

that we thus obtain a diverging lens, the curvature and action of which are variable.

23. Now let us suppose that we have increased the curvatures of the lens until the two surfaces nearly touch each other by their summits.* We might presume that if the removal of the liquid were continued, the mass would become disunited at that point at which this contact took place, and that the oil would recede in every direction towards the metallic band. This is, however, not the case; we then observe in the centre of the figure the formation of a small sharply defined circular space, through which objects no longer appear diminished, and we easily recognize that this minute space is occupied by a layer of oil with plane faces. If the removal of the liquid be gradually continued, this layer increases more and more in diameter, and may thus be extended to within a tolerably short distance of the solid surface. In my experiment, the diameter of the metallic cylinder was seven centimetres, and I have been enabled to increase the size of the layer until its circumference was not more than about five millimetres from the solid surface; but at this instant it broke, and the liquid of which it consisted rapidly receded towards that which still adhered to the metallic band. The fact which we have just described is very remarkable, both in itself and in the singular theoretical consequences to which it leads. In fact, that part of the mass to which the layer adheres by its margin presents concave surfaces, whilst those of the layer are plane; now the existence of such a system of surfaces in a continuous liquid mass seems in opposition to theory, since it appears evident that the pressures cannot be equal in this case. But let us investigate the question more minutely.

24. According to theory, the pressure corresponding to any point of the surface of a liquid mass, as we have seen, (§ 3,) is the integral of the pressures exerted by each of the molecules composing a rectilinear line perpendicular to the surface at that point, and equal in length to the radius of the sphere of activity of the molecular attraction. The analytical expression of this integral contains no other variables than the radii of the greatest and of the least curvature at the point under consideration, (§ 4,) consequently the pressure in question varies only with the curvatures of the surface at the same point. This is rigorously true when the liquid is of any notable thickness; but we shall show that in the case of an extremely thin layer of liquid there is another element which exerts an influence upon the pressure. Let us conceive a liquid layer, the thickness of which is less than twice the radius of the sphere of sensible activity of the molecular attraction. Let each molecule be conceived to be the centre of a small sphere with this same radius, (§ 3,) and let us first consider a molecule situated in the middle of the thickness of the layer. The little sphere, the centre of which is occupied by this molecule, will be intersected by the two surfaces of the layer, consequently it will not be entirely full of liquid; but the segments suppressed on the outside of the two surfaces being equal, the molecule will not be more attracted perpendicularly in one direction than in the other. Now let a small right line, normal to and terminating at the two surfaces, pass through this same molecule, and let us consider a second molecule situated at some other point of this right line. The little sphere which belongs to the second molecule in question may again be intersected by the two surfaces of the layer; but then the two suppressed segments will be unequal; the molecule will consequently be subjected to a preponderating attraction, evidently directed towards the thickness of the layer. The molecule will then exert a pressure in this direction, and it must be remarked that this pressure will be less than if the liquid had any notable thickness, the molecule

* To effect this operation, the point of the syringe must not be placed in the middle of the figure, as in the case of the doubly convex lens; but, on the contrary, near the metallic band, as this is now the point where the greatest thickness of the liquid exists.

being situated at the same distance from the surface; for in the latter case the little sphere would only be cut on one side, and its opposite part would be perfectly full of liquid. It might also happen that the little sphere belonging to the molecule in question in the thin layer is only cut on one side; the molecule will then still exert a pressure in the same direction, but its intensity will then be as great as in the case of a thick mass. It is easy to see that if the thickness of the layer is less than the simple length of the radius of the molecular attraction, the little spheres will all be cut on both sides; whilst if the thickness in question is comprised between the length of the above radius and twice this same length, a portion of the minute spheres will be cut on one side only. In both cases the pressure exerted by any molecule being always directed towards the middle of the thickness of the layer, it is evident that the integral pressure corresponding to any point of either of the two surfaces will be the result of the pressures individually exerted by each of those molecules, which, commencing at the point in question, are arranged upon half the length of the small perpendicular. Now each of the two halves of the small perpendicular being less than the radius of the sphere of activity of the molecular attraction, it follows that the number of molecules composing the line which exerts the integral pressure is less than in the case of a thick mass. Thus, on the one hand, the intensities of part or the whole of the elementary pressures composing the integral pressure will be less than in the case of a thick mass, and, on the other hand, the number of these elementary pressures will be less; from this it evidently follows that the integral pressure will be inferior to that which would occur in the case of a thick mass. P always denoting the pressure corresponding to any point of a plane surface belonging to a thick mass, (§ 4,) the pressure corresponding to any point of either of the surfaces of an extremely thin plane layer will therefore be less than P. Moreover, this pressure will be less in proportion as the layer is thinner, and it may thus diminish indefinitely; for it is clear that it would be reduced to zero if we supposed that the thickness of the layer was equal to no more than that of a simple molecule.

We can obtain liquid layers with curved surfaces; soap-bubbles furnish an example of these, and we shall meet with others in the progress of this investigation. Now by supposing the thickness of such a layer to be less than twice the radius of the molecular attraction, we should thus evidently arrive at the conclusion that the corresponding pressures at either of its two surfaces would be inferior in intensity to those given by paragraph 4, and that, moreover, these intensities are less in proportion as the layer is smaller. We thus arrive at the following new principle:

In the case of every liquid layer, the thickness of which is less than twice the radius of the sphere of activity of the molecular attraction, the pressure will not depend solely upon the curvatures of the surfaces, but will vary with the thickness of the layer.

25. We thus see that an extremely thin plane liquid layer, adhering by its edge to a thick mass the surfaces of which are concave, may form with this mass a system in a state of equilibrium; for we may always suppose the thickness of the layer to be of such value that the pressure corresponding to the plane surfaces of this layer is equal to that corresponding to the concave surfaces of the thick mass. Such a system is also very remarkable in respect to its form, inasmuch as surfaces of different nature, as concave and plane surfaces, succeed each other. This heterogeneity of form is, moreover, a natural consequence of the change which the law of pressures undergoes in passing from the thick to the thin part.

26. As we have already seen, theory demonstrates the possibility of the existence of such a system in a state of equilibrium. As regards the experiment which has led us to these considerations, although the result presented by it tends to realize in an absolute manner the theoretical result, there is one circum-

stance which is unfavorable to the completion of this realization. We can understand that the relative mobility of the molecules of oil is not sufficiently great to occasion the immediate formation of the liquid layer with that excessive tenuity which is requisite for equilibrium; the thickness of this layer, although very minute, absolutely speaking, is undoubtedly, during the first moments, a considerable multiple of the theoretical thickness. If, then, we produce the layer without extending it to that limit to which it is capable of increasing during the operation, and afterwards leave it to itself, the pressure corresponding to its plane surfaces will still exceed that corresponding to the concave surfaces of the remainder of the liquid system. Hence it follows that the oil within the layer will be driven towards this other part of the system, and that the thickness of the layer will progressively diminish. The equilibrium of the figure will then be apparent only, and the layer will in reality be the seat of continual movements. The diminution in thickness, however, will be effected slowly, because in so confined a space the movements of the liquid are necessarily restrained; this is why, as in the experiment in paragraph 17, the mass only acquires its figure of equilibrium slowly, because there is a cause which impedes the movements of the liquid. The thickness of the layer gradually approximates to the theoretical value, from which the equilibrium of the system would result; but unfortunately it always happens that before attaining this point the layer breaks spontaneously. This effect depends, without doubt, upon the internal movements of which I have spoken above. We can imagine, in fact, that when the layer has become of extreme thinness, the slightest cause is sufficient to determine its rupture. The exact figure which corresponds to the equilibrium is therefore a limit towards which the figure produced tends; this limit the latter approaches very nearly, and would attain if it were not itself previously destroyed by an extraneous cause.

Our experiment has led us to modify the results of theory in one particular instance; but we now see that, far from weakening the principles of this theory, it furnishes, on the contrary, incomplete as it is, a new and striking verification of it. The conversion of the doubly concave lens into a system comprising a thin layer is connected with an order of general facts: we shall see that a large number of our liquid figures become transformed, by the gradually produced diminution of the mass of which they are composed, into systems consisting of layers, or into the composition of which layers enter.

27. If by some modification of our last experiment we could succeed in obtaining the equilibrium of the liquid system, we might be able to deduce from it a result of great interest—an indication of the value of the radius of the sphere of activity of the molecular attraction. In fact, we might perhaps find out some method of determining the thickness of the layers; these might, for instance, then exhibit colors, the tint of which would lead us to this determination. Now we have seen that in the state of equilibrium of the figures, half the thickness of the layer would be less than the radius in question; hence we should then have a limit above which the value of this same radius would exist. In other words, we should know that the molecular attraction produces sensible effects, even at a distance from its centre of action beyond this limit. Our experiment, although insufficient, may thus be considered as the first step towards the determination of the distance of sensible activity of the molecular attraction, of which distance at present we know nothing, except that it is of extreme minuteness.

28. Let us now return to the consideration of thick masses. It follows from the experiments related in paragraphs 13, 14, 17, 18, and 21, that when a continuous portion of the surface of such a mass rests upon a circular periphery, this surface is always either of spherical curvature or plane. But to admit this principle in all its generality, we must be able to deduce it from theory. We shall do this in the following series, at least on the supposition that the portion

of the surface in question is a surface of revolution. We shall then see that this same principle is of great importance. We may remark here that in the experiment in paragraph 23 the layer commences to appear as soon as the surfaces can no longer constitute spherical segments. Now we shall again find that in the other cases, when a full figure is converted, by the gradual withdrawal of the liquid, into a system composed of layers, or into the composition of which layers enter, the latter begin to be formed when the figure of equilibrium, which the ordinary law of pressures would determine, ceases to be possible. The mass then assumes, or tends to assume, another figure, compatible with a modification of this law. Such is the general principle of the formation of layers under the circumstances in question.

Fig. 8.

29. After having formed a converging and a diverging liquid lens, it appeared to me curious to combine these two kinds of lens, so as to form a liquid telescope. For this purpose, I first substituted for the ring of iron wire, in paragraph 18, a circular plate of the same diameter, perforated by a large aperture. (Fig. 8.) This plate having been turned in a lathe, I was certain of its being perfectly circular, which would be a very difficult condition to fulfil in the case of a simple curved iron wire. In the second place, I took for the solid part of the doubly concave lens a band of about two centimetres in breadth, and curved into a cylinder three and a half centimetres in diameter. These two systems were arranged as in Fig. 9, in such a manner that the entire apparatus being

Fig. 9.

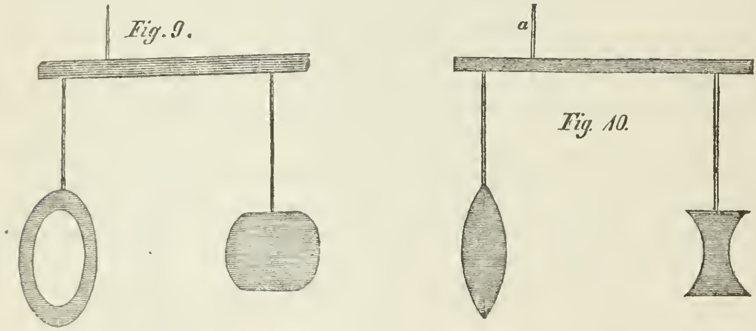


Fig. 10.

suspended vertically in the alcoholic mixture by the iron wire a, and the two liquid lenses being formed, their two centres were at the same height, and ten centimetres distant from each other. In this arrangement the telescope cannot be adjusted by altering the distance between the objective and the eye-piece; but this end is attained by varying the curvatures of these two lenses. With the aid of a few preliminary experiments, I easily managed to obtain an excellent Galilean telescope, magnifying distant objects about twice, like a common opera-glass, and giving perfectly distinct images with very little irisation. Fig. 10, which represents a section of the system, shows the two lenses combined.

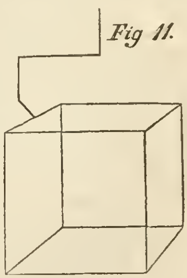
Figures of equilibrium terminated by plane surfaces. Liquid polyhedra. Laminar figures of equilibrium.

30. In the experiment detailed at paragraph 21, we obtained a figure presenting plane surfaces. These were two in number, parallel, and bounded by circular peripheries; but it is evident that these conditions are not necessary

in order to allow plane surfaces to belong to a liquid mass in equilibrium. We can understand that the forms of the solid contours might be indifferent, provided they constitute plane figures. We can, moreover, understand that the number and the relative directions of the plane surfaces may be a matter of indifference, because these circumstances exert no influence upon the pressures which correspond to these surfaces, pressures which will always remain equal to each other. Lastly, it follows from the principle at which we arrived at the end of paragraph 20, relative to the influence of solid wires, that for the establishment of the transition between a plane and any other surface, a metallic thread representing the edge of the angle of intersection of these two surfaces will be sufficient. We are thus led to the curious result, that we ought to be able to form polyhedra, which are entirely liquid excepting at their edges. Now, this is completely verified by experiment. If for the solid system we take a framework of iron wire representing all the edges of any polyhedron, and we cause a mass of oil of the proper volume to adhere to this framework, we obtain, in fact, in a perfect manner, the polyhedron in question; and the curious spectacle is thus obtained of parallelopipedons, prisms, &c., composed of oil, and the only solid part of which is their edges.

To produce the adhesion of the liquid mass to the entire framework, a volume is first given to the mass slightly larger than that of the polyhedron which it is to form; it is then placed in the framework; and, lastly, by means of the iron spatula, (§ 9,) which must be introduced by the second aperture of the lid of the vessel, and which is made to penetrate the mass, the latter is readily made to attach itself successively to the entire length of each of the solid edges. The excess of oil is then gradually removed with the syringe, and all the surfaces thus become simultaneously exactly plane. But that this end may be attained in a complete manner, it is clearly requisite that the equilibrium of density between the oil and the alcoholic mixture should be perfectly established; and the slightest difference in this respect is sufficient to alter the surfaces sensibly. It should also be borne in mind that the manipulation with the spatula sometimes occasions the introduction of alcoholic bubbles into the interior of the mass of oil. These are, however, easily removed by means of the syringe.

Fig 11.



31. Now, having formed a polyhedron, let us see what will happen if we gradually remove some of the liquid. Let us take, for instance, the cube, the solid framework of which, with its suspending wire, is represented at Fig. 11.* Let the point of the syringe be applied near the middle of one of the faces, and let a small quantity of the oil be drawn up. All the faces will immediately become depressed simultaneously and to the same extent, so that the superficial square contours will form the bases of six similar hollow figures. We should have imagined this to have been the case for the maintenance of equality between the pressures.

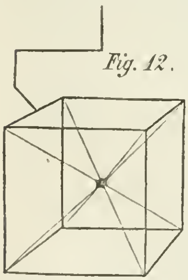
If fresh portions of the liquid are removed, the faces will become more and more hollowed; but to understand what happens when this manipulation is continued, we must here enunciate a preliminary proposition. Suppose that a square plate of iron, the sides of which are of the same length as the edges of the metallic frame, is introduced into the vessel, and that a mass of oil equal in volume to that which is lost by one of the faces of the cube is placed in contact with one of the faces of this plate; I say that the liquid, after having become extended upon the plate, will present in relief the same figure as the

* The edges of all the frames which I used were 7 centimetres in length.

face of the modified cube presents *in intaglio.* Then, in fact, in passing from the hollow surface to that in relief, the radii of curvature corresponding to each point will only change their signs without changing in absolute value; consequently, (§ 8,) since the condition of equilibrium is satisfied as regards the first of these surfaces, it will be equally so with regard to the second.

Now, let us imagine a plane passing through one side of the plate, and tangentially to the surface of the liquid which adheres to it at that point. As long as this liquid is in small quantity, we should imagine—and experiment bears us out—that the plane in question will be strongly inclined towards the plate; but if we gradually increase the quantity of liquid, the angle comprised between the plane and the plate will also continue to increase, and instead of being acute, as before, will become obtuse. Now, so long as this angle is less than 45°, the convex surface of the liquid adhering to the plate will remain identical with the concave surfaces of the mass attached to the metallic frame, and suitably diminished; but beyond this limit, the coexistence in the frame of the six hollow identical surfaces with the surface in relief becomes evidently impossible, for these surfaces must mutually intersect each other. Thus, when the withdrawal of the liquid from the mass forming the cube is continued, a point is attained at which the figure of equilibrium ceases to be realizable in accordance with the ordinary law of pressures. We then meet with a new verification of the principle enunciated in § 28, *i. e.*, that the formation of layers commences. These layers are plane; they commence at each of the wires of the frame, and connect the remainder of the mass to the latter, which continues to present six concave surfaces. In fact, we can imagine that, by this modification of the liquid figure, the existence of the whole of this in the metallic frame again becomes possible, as also the equilibrium of the system; for there is then no further impediment to the concave surfaces assuming that form which accords with the ordinary law of pressures; and, on the other hand, in supposing the layers to be sufficiently thin, the pressure belonging to them might be equal to that which corresponds to these same concave surfaces, (§ 25.)

Fig. 12.



On removing still further portions of the liquid, the layer will continue to enlarge, whilst the full mass which occupies the middle of the figure will diminish in volume, and this mass can thus be reduced to very minute dimensions: Fig. 12 represents the entire system in this latter state. It is even possible to make the little central mass disappear entirely, and thus to obtain a complete laminar system; but for this purpose certain precautions must be taken, which I shall now point out. When the central mass has become sufficiently small, the point of the syringe must first be thoroughly wiped; otherwise the oil adheres to its exterior to a certain height, and this attraction keeps a certain quantity of oil around it, which the instrument cannot absorb into its interior. In the second place, the point of the syringe must be depressed to such an extent that it nearly touches the inferior surface of the little mass. During the suction this surface is then seen to become raised, so as to touch the orifice of the instrument, and the latter then absorbs as much of the alcoholic mixture as of the oil; but this is of no consequence, and the minute mass is seen to diminish by degrees, so as at last completely to disappear. The system, then, consists of twelve triangular layers, each of which commences at one of the wires of the frame, and all the summits of which unite at the centre of the figure; it is represented in Fig. 13. But this system is only formed during the action of the syringe. If, when this is complete, the point of the instrument is slowly withdrawn, an additional lamina of a square form is seen

to be developed in the centre of the figure, (Fig. 14.) This, then, is the definitive laminar system to which the liquid cube is reduced by the gradual diminution of its mass.

Fig 13

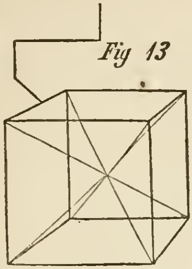
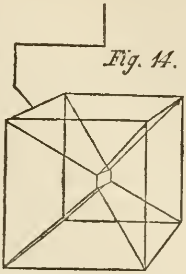


Fig. 14.



32. In the preceding experiment, as in that of paragraph 23, the thickness of the layers is at first greater than that which would correspond to equilibrium. If, then, the system were left to itself whilst it still contains a central mass, we should imagine that one portion of the liquid of the layers would be slowly driven towards this mass, and that the layers would gradually become thinner. Moreover, it always happens that one or the other of the latter increases after some time, undoubtedly for the reason which we have already pointed out, (§ 26.) Hence, for the perfect success of the transformation of the cube into the laminar system, one precaution, which has not yet been spoken of, must be attended to. It consists in the circumstance that, from the instant at which the layers arise, the exhaustion of the liquid must be continued as quickly as possible until the central mass has attained a certain degree of minuteness. In fact, as soon as the formation of the layers commences, their tendency to become thinner also begins to be developed; and if the operation is effected too slowly, the system might break before it was completed. When the central mass is sufficiently reduced—and experience soon teaches us to judge of the suitable point—the action of the syringe must be gradually slackened, and at last the other precautions which we have mentioned must be taken.

We are able, then, to explain the rupture of the layers so long as there is a large or small central mass; but when the laminar system is complete, we do not at the first glance see the reason why the thickness of the layers diminishes, and consequently why destruction of the system takes place. Nevertheless the rupture ultimately takes place in this as in the other case, and the time during which the system persists rarely extends to half an hour. In ascertaining the cause of this phenomenon, it must be remarked that the intersections of the different layers cannot occur suddenly, or be reduced to simple lines: it is evident that the free transition between two liquid surfaces could not be thus established in a discontinuous manner. These transitions must, therefore, be effected through the intermedium of minute concave surfaces, and with a little attention we can recognize that, in fact, this really takes place. We can then understand that the oil of the layers ought also to be driven towards the places of junction of the latter; and consequently the absence of the little central mass does not prevent the gradual attenuation of the layers, and the final destruction of the system.

33. If, during the action of the syringe, when the system shown in Fig. 13 has been attained, instead of slowly withdrawing the instrument, it is suddenly detached by a slight shake in a vertical direction, the additional layer is not developed; but the little mass in Fig. 12 is seen to be reproduced very rapidly. This fact confirms in a remarkable manner the explanation which we have given in the preceding paragraph. In fact, at the moment at which the point of the instrument is separated from the system, the latter may be considered as composed of hollow pyramids. Now it also follows, from causes relating to

their continuity, that the summits of these pyramids should not constitute simple points, but little concave surfaces. But as the curvatures of these minute surfaces are very great in every direction, they would give rise to still far less pressure than those which establish the transitions between each pair of surfaces of the layers; for in the latter there is no curvature in one direction. The oil of the layers will, therefore, be driven with much greater force towards the centre of the figure than towards the other parts of the junctions of these layers. Again, the twelve layers terminating in this same centre, the oil flows there simultaneously from a large number of sources. These two concurrent causes ought then, in conformity with experiment, to produce the rapid reappearance of the small central mass; and we can understand why it is impossible to obtain the complete system of the pyramids otherwise than during the action of the syringe.

34. All the other polyhedric liquids become transformed, like the cube, into laminar systems when the mass of which they are composed is gradually diminished. Among these systems some are complete; the others still contain very small masses, which cannot be made to disappear entirely. Analogous considerations to those which we applied with regard to the cube would show, in each case, that the formation of layers commences as soon as the hollow surfaces which would correspond to the ordinary law of pressures cease to be able to coexist in the solid frame. Figs. 15, 16, 17, and 18 represent the

Fig. 15.

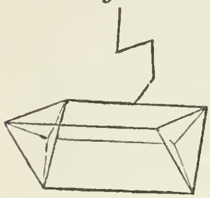


Fig. 16.

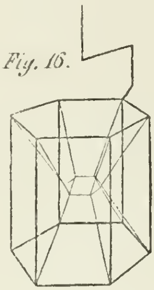


Fig. 17.

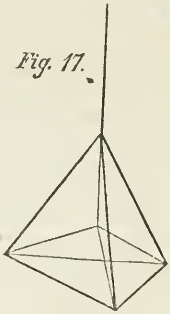
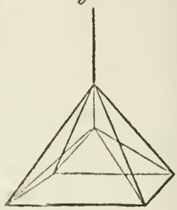


Fig. 18



laminar systems resulting from the triangular prism, the hexahedral prism, the tetrahedron and the pyramid with a square base, these systems being supposed to be complete. They are all formed of plane layers, commencing at each of the metallic wires; and that of the hexahedral prism, as is shown, contains an additional layer in the centre of the figure.

Fig. 19.

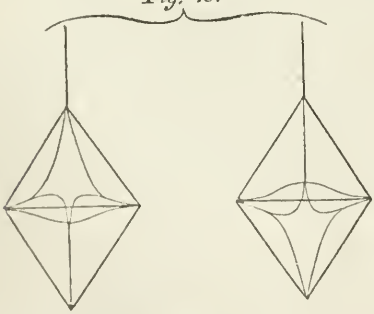
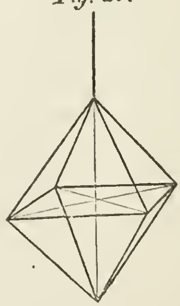


Fig. 20.



35. The system arising from the regular octohedron presents a singular exception, which I have not been able to explain. The layers of which this system is composed are curved, and form a fantastical group, of which it is difficult to give an exact idea by graphic representations. Fig. 19 exhibits

them projected upon two rectangular vertical planes; and it is seen that the aspects of the system observed upon two adjacent sides are inverse as regards each other. The formation of this system presents a curious peculiarity. At the commencement of the operation all the faces of the octohedron become simultaneously hollow; the layers in progress of formation are plane, and arranged symmetrically, so that the system tends towards the form represented at Fig. 20. But when a certain limit is attained, a sudden change occurs, the layers become curved, and the system tends to assume the singular form which we have mentioned. I have several times repeated the experiment, varying the circumstances as much as possible, and the same effects are always produced.

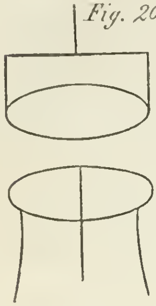
In the course of this memoir I shall point out another process for obtaining laminar systems; it is an extremely simple one, and has moreover the advantage of producing all the systems in a complete state.

36. In concluding our observations upon polyhedric liquids, I shall remark that the triangular prism may be employed to produce the phenomena of dispersion. In this way a beautiful solar spectrum may be obtained by means of a prism with liquid faces. But as the effect only depends upon the excess of the refracting action of the oil above that of the alcoholic liquid, to obtain a considerably extended spectrum the angle of refraction of the prism must be obtuse; an angle of 110° gives a very good result. Moreover, it is evidently requisite that the faces of the prism should be perfectly plane, which is obtained by using a carefully made frame; by establishing exact equilibrium between the density of the liquids; and, lastly, by arresting the action of the syringe exactly at the proper point.

Other figures of Revolution besides the Sphere. Liquid Cylinder.

37. Let us now endeavor to form some new liquid figures. Those best adapted to theoretical considerations would be figures terminated by surfaces of revolution other than the sphere and lenticular figures, which we have already studied. Surfaces of revolution enjoy simple properties in regard to the radii of the greatest and least curvature at every point; we know that one of these two radii is the radius of curvature of the meridional line, and that the other is that portion of the normal to this line which is included between the point under consideration and the axis of revolution. We shall now endeavor to obtain figures of this nature.

*Fig. 20.**



38. Let our solid system be composed of two rings of iron wire, equal, parallel, and placed opposite to each other. One of these rings rests upon the base of the vessel by three feet composed of iron wire; the other is attached, by means of an intermediate piece, to the axis traversing the central stopper, so that it may be approximated to or removed from the former by depressing or elevating this axis.* The system formed by these two rings is represented in Plate VII, Fig. 20 *bis;* the diameter of those which I employed was 7 centimeters.

After having raised the upper ring as much as possible, let a sphere of oil, of a slightly larger diameter than that of the rings, be formed, and conducted towards the lower ring in such a manner as to make it adhere to the entire circumference of the latter; then depress the upper ring until it comes into contact with the liquid mass, and the latter is uniformly attached to it. When the mass has thus become adherent to the system of the two rings, let the upper ring be slowly raised; when the two rings are at a proper distance apart, the liquid will then assume the form the vertical projection of which is represented in Fig. 21, in which the lines $a\ b$ and $c\ d$ are the projections of the rings. The two portions of the surface which are respectively applied to each of the rings are convex spherical segments; and the portion included between the two rings constitutes a figure of revolution, the meridional curve of which, as is shown, is convex externally. We shall recur, in the following series, to this part of the liquid figure. If we now continue gradually to raise the upper ring, the curvature of the two extremities and the meridional curvature of the intermediate portion will be diminished; and if there is exact equilibrium between the density of the oil and the surrounding liquid, the surface included between the two rings will be seen to assume a perfectly cylindrical form, (Fig. 22.) The two bases of the liquid figure are still convex spherical segments, but their curvature is less than in the preceding figure. If the interval between the rings be still further increased, it is evident that the surface included between them would lose the cylindrical form, and that a new figure would result. This is what occurs; but the consideration of the figure thus produced must be deferred.

Fig. 21.

Fig. 22.

Instead, then, of immediately increasing the distance between the rings, let us commence by adding a certain quantity of oil to the mass, which will again

* In the experiments which we are now about to describe, the short axis represented in Fig. 2 of the preceding memoir, and which has hitherto answered our purpose, must be replaced by another of about 15 centimeters in length.

render the surface included between the rings convex. Let us then gradually elevate the upper ring, and we shall produce a cylinder of greater height than

Fig. 23.

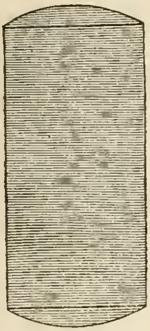
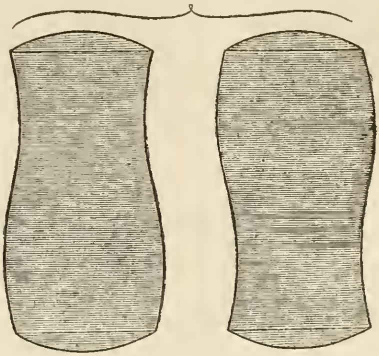


Fig. 24.



the first. If we repeat the same manipulation a suitable number of times, we shall ultimately obtain the cylinder of the greatest height which our apparatus permits. I have in this manner obtained a perfectly cylindrical mass 7 centimeters in diameter, and about 14 centimeters in height, (Fig. 23.) To allow of the cylinder of this considerable height being perfect, it is requisite that perfect equality be established between the densities of the oil and the alcoholic liquid. As a very slight difference in either direction tends to make the mass ascend or descend, the latter assumes, to a more or less marked extent, one of the two forms represented in Fig. 24. Even when the cylindric form has been obtained by the proper addition of alcohol of 16°, or absolute alcohol, as occasion may require, (§ 24 of the preceding memoir,) slight changes in temperature are sufficient to alter and reproduce one of the above two forms.

39. Let us now examine the results of these experiments in a theoretical point of view. First, it is evident that a cylindrical surface satisfies the general condition of equilibrium of liquid figures, because the curvatures in it are the same at every point. Moreover, such a surface being convex in every direction except in that of the meridional line, where there is no curvature, the pressure corresponding to it ought to be greater than that corresponding to a plane surface. The same conclusions are deducible from the general formulæ (2) and (3) of paragraphs 4 and 5. In fact, as we have already stated in paragraph 37, one of the quantities R and R′ is the radius of curvature of the meridional line, and the other is the portion of the normal to this line included between the point under consideration and the axis of revolution. Now, in the case of the cylinder, the meridional line being a right line, its radius of curvature is everywhere infinitely great; and, on the other hand, this same right line being parallel to the axis of revolution, that portion of the normal which constitutes the second radius of curvature is nothing more than the radius itself of the cylinder. Hence it follows that one of the terms of the quantity $\frac{1}{R}+\frac{1}{R'}$ disappears, and that the other is constant; this same quantity is, therefore, constant, and consequently the condition of equilibrium is satisfied. Now, if we denote by λ the radius of the cylinder, the general value of the pressure for this surface would become

$$P+\frac{A}{1}\cdot\frac{1}{\lambda}.$$

Now λ being positive because it is directed towards the interior of the liquid, (§ 4,) the above value is greater than P, *i. e.*, than that which would correspond

to a plane surface. It is, therefore, evident that the bases of our liquid cylinder must necessarily be convex, as is shown to be the case by experiment; for as equilibrium requires that the pressures should be the same throughout the whole extent of the figure, these bases must produce a greater pressure than that which corresponds to a plane surface.

Our plane figure, then, fully satisfies theory; but verification may be urged still further. Theory allows us to determine with facility the radius of those spheres of which the bases form a part. In fact, if we represent this radius by x, the formula (1) of paragraph 4 will give, for the pressure corresponding to the spheres in question,

$$P + A \cdot \frac{1}{x}.$$

Now, as this pressure must be equal to that corresponding to the cylindrical surface, we shall have

$$P + \frac{A}{2} \cdot \frac{1}{\lambda} = P + A \cdot \frac{1}{x},$$

from which we may deduce

$$x = 2\lambda.$$

Thus the radius of the curvature of the spherical segments constituting the bases is equal to the diameter of the cylinder.

Hence, as we know the diameter, which is the same as that of the solid rings, we may calculate the height of the spherical segments; and if by any process we afterwards measure this height in the liquid figure, we shall thus have a verification of theory even as regards the numbers. We shall now investigate this subject.

40. If we imagine the liquid figure to be intersected by a meridional plane, the section of each of the segments will be an arc belonging to a circle, the radius of which will be equal to 2λ, according to what we have already stated, and the versed sine of half this arc will be the height of the segment. If we suppose the metallic filaments forming the rings to be infinitely small, so that each of the segments rests upon the exact circumference of the cylinder, the chord of the above arc will also be equal to 2λ; and if we denote the height of the segments by h, we shall have

$$h = \lambda(2 - \sqrt{3}) = 0.268 \,.\, \lambda.$$

Now, the exact external diameter of my rings, or the value of 2λ, corresponding with my experiments, was 71.4 millimeters, which gives $h = 9.57$ millimeters. But as the metallic wires have a certain thickness, and the segments do not rest upon the external circumference of the rings, it follows that the chord of the meridional arc is a little less than 2λ, and that, consequently, the true theoretical height of the segments is a little less than that given by the preceding formula. To determine it exactly, let us denote the chord by $2c$, which will give

$$h = 2\lambda - \sqrt{4\lambda^2 - c^2}.$$

Now, let us remark that the meridional plane intersects each of the rings in two small circles to which the meridional arc of the spherical segment is tangential, and upon each of which the chord of this arc intercepts a small circular segment. The meridional arc being tangential to the sections of the wire, it follows that the above small circular segments are similar to that of the spherical segment; and as the chord of the latter differs but very slightly from the radius of the circle to which the arc belongs, the chords of the small circular segments may be considered as equal to the radius of the small sections, which radius we shall denote by r. It is moreover evident that the excess of the external radius of the ring over half the chord c is nothing more than the excess

of the radius r over half the chord of the small circular segments, which half chord, in accordance with what we have stated, is equal to $\frac{1}{2}r$. Thence we get $\lambda - c = \frac{1}{2}r$, whence $c = \lambda - \frac{1}{2}r$, and we have only to substitute this value in the preceding formula to obtain the true theoretical value of h. The thickness of the wire forming my rings is 0.74 millimeters; hence $\frac{1}{2}r = 0.18$ millimeters, which gives as the true theoretical height of the segments under these circumstances,

$$h = 9.46 \text{ millimeters.}$$

I may remark that it is difficult to distinguish in the liquid figure the precise limit of the segments, *i. e.*, the circumferences of contact of their surfaces with those of the rings. To get rid of this inconvenience, I measured the height of the segments, commencing only at the external planes of the rings; *i. e.*, in the case of each segment, commencing at a plane perpendicular to the axis of revolution, and resting upon the surface of the ring on that side which is opposite the summit of the segment. The quantity thus measured is evidently equal to the total height minus the versed sine of the small circular segments which we have considered above; consequently these small circular segments being similar to that of the spherical segment, we obtain for the determination of this versed sine, which we shall denote by f, the proportion $\frac{h}{c} = \frac{f}{\frac{1}{2}r}$, which in the case of our liquid figure gives $f = 0.05$ millimeters, whence

$$h - f = 9.41 \text{ millimeters.}$$

This, then, is definitively the theoretical value of the quantity which was required to be measured.

41. Before pointing out the process which I employed for this purpose, and communicating the result of the operation, I must preface a few important remarks. If the densities of the alcoholic mixture and of the oil are not rigorously equal, the mass has a slight tendency to rise or descend, and the height of one of the segments is then a little too great, whilst that of the other is a little too small; but we can understand that if their difference is very small, an exact result may still be obtained by taking the mean of these two heights. We thus avoid part of those preliminary experiments which the establishment of perfect equality between the two densities requires. But one circumstance which requires the greatest attention is the perfect homogeneity of each of the two liquids. If this condition be not fulfilled with regard to the alcoholic mixture, *i. e.*, if the upper part of this mixture be left containing a slightly greater proportion of alcohol than the lower portion, the liquid figure may appear regular and present equal segments; all that is required for this is, that the mean density of that part of the mixture, which is at the same level as the mass, must be equal to the density of the oil; but under these circumstances the level of the two segments is too low. In fact, the oil forming the upper segment is then in contact with a less dense liquid than itself, and, consequently, has a tendency to descend, whilst the opposite applies to the oil forming the inferior segment.* Heterogeneity of the liquid produces an opposite effect, *i. e.*, it renders the height of the segments too great. In fact, the least dense portions rising to the upper part of the mass tend to lift it up, whilst the most dense portions descend to the lower part, and tend to depress it. Now,

* By intentionally producing very great heterogeneity in the alcoholic mixture, (§ 9 of the preceding memoir,) and employing suitable precautions, a perfectly regular cylinder may be formed, the bases of which are absolutely plane.

the quantities of pure alcohol, and that at 16° added to the alcoholic mixture to balance the mass, necessarily produce an alteration in the homogeneity of the oil; for, in the first place, the oil during these operations being in contact with mixtures which are sometimes more, sometimes less charged with alcohol, must absorb or lose some of this by its surface; in the second place, these same additions of alcohol to the mixture diminish the saturation of the latter with the oil, so that it removes some of it from the mass; and this action is undoubtedly not equally exerted upon the two principles of which the oil is composed. Hence, before taking the measures, the different parts of the oil must be intimately mixed together, which may be effected by introducing an iron spatula into the mass, moving it about in it in all directions, and this for a long time, because the mixture of the oil can only be perfectly effected with great difficulty on account of its viscidity.

To avoid the influence of the reactions which render the oil heterogeneous, the operations must be conducted in the following manner: The mass being introduced into the vessel and attached to the two rings, and the equality of the densities being perfectly established, allow the mass to remain in the alcoholic liquid for two or three days, re-establishing from time to time the equilibrium of the densities altered by the chemical reactions and the variations of temperature. Afterwards remove the two rings from the vessel, so that the mass remains free; remove almost the whole of this, by means of a siphon, into a bottle, which is to be carefully corked; withdraw with the syringe the small portion of oil which is left in the vessel, and reject this portion. Next replace the two rings, and mix the alcoholic liquid perfectly; then again introduce the oil into the vessel, taking the precaution of enveloping the bottle containing it with a cloth several times folded, so that the temperature may not be sensibly altered by the heat of the hand.* Then attach the mass to the lower ring only, the upper ring being raised as much as possible; mix the oil intimately, as we have said above; then depress the upper ring, cause the mass to adhere to it, elevate it so as to form an exact cylinder, and proceed immediately to the measurement.

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* The following is the reason why the oil must be removed from the vessel before employing it for the experiment. After having remained a considerable time in the alcoholic liquid, the oil becomes enveloped by a kind of thin pellicle; or, more strictly speaking, the superficial layer of the mass has lost part of its liquidity, an effect which undoubtedly arises from the unequal action of the alcohol, upon the principles of which the oil is composed. The necessary result of this is, that the mass loses at the same time part of its tendency to assume a determinate figure of equilibrium, which tendency must, therefore, be completely restored to it. This is why the oil is withdrawn by the siphon. In fact, the pellicle does not penetrate the interior of the latter, and during its contraction continues to envelop the small portion remaining; so that after the latter has been removed by the syringe, which ultimately absorbs the pellicle itself, we get completely rid of the latter.

Before using the siphon, the thickness and consistence of the pellicle are too slight to enable us distinctly to perceive its presence; but when the operation of the siphon is nearly terminated, and the mass is thus considerably reduced, we find that the surface of the latter forms folds, hence implying the existence of an envelope. Moreover, when the siphon is removed, the small residuary mass, which then remains freely suspended in the alcoholic liquid, no longer assumes a spherical form, but retains an irregular aspect, appearing to have no tendency to assume any regular form.

This indifference to assume figures of equilibrium, arising from a diminution in the liquidity of the superficial layer, constitutes a new and curious proof of the fundamental principle relating to this layer, (§§ 6 *bis* and 10 to 16.) M. Hagen (*Mémoire sur la Surface des Liquides*, in the Memoirs of the Academy of Berlin, 1845) has observed a remarkable fact, to which the preceding appears to be related. It consists in this, that the surface of water, left to itself for some time, undergoes a peculiar modification, in consequence of which the water then rises in capillary spaces to elevations which are very distinctly less than is the case when its surface is exempt or freed from this alteration. This fact might, perhaps, be explained by admitting that the water dissolves a small proportion of the substance of the solid with which it is in contact, and that the external air acts chemically at the surface of the liquid upon the substance dissolved, thus giving rise to the formation of a slight pellicle which modifies the effects of the molecular forces.

42. The instrument best suited for effecting the latter operations in an exact manner is undoubtedly that which has received the name of *cathetometer*, and which, as is well known, consists of a horizontal telescope moving along a vertical divided rule. The distance comprised between the summits of the two segments is first measured by the aid of this instrument; the distance included between the external planes of the two rings (§ 40) is then measured by the same means. The difference between the first and the second result evidently gives the sum of the two heights, the mean of which must be taken; and, consequently, this mean, or the quantity sought, $h-f$, is equal to half the difference in question.

The determination of the distance between the external planes of the rings requires peculiar precautions. First, as the points of the rings at which we must look are not exactly at the external surface of the figure, the oil interposed between these points and the eye must produce some effects of refraction, which would introduce a slight error into the value obtained. To avoid this inconvenience, we need only expose the rings by allowing the liquids to escape from the vessel by the stop-cock, (note 2 to § 9,) then remove the minute portions of the liquid which remain adherent to the rings by passing lightly over their surface a small strip of paper, which must be introduced into the vessel through the second aperture. The drops of alcoholic liquid remaining attached to the inner surface of the interior side of the vessel must also be absorbed in the same manner. In the second place, as it would be difficult for the rings to be rigorously parallel, their distance must be measured from two opposite sides of the system, and the mean of the two valves thus found taken. The following are the results which I obtained: The mensuration of the distance between the summits gave first, in four successive operations, the values 76.77, 76.80, 76.85, and 76.75 millimeters, the mean of which is 76.79 millimeters. But after the alcoholic liquid had been again agitated for some time, to render its homogeneity more certain, two new measurements taken immediately afterwards gave 77.05 and 77.00 millimeters, or a mean of 77.02 millimeters. The distance between the external planes of the rings was found, on the one hand, by two observations, which agreed exactly, to be 57.73 millimeters; on the other hand, two observations furnished the values 57.87 and 57.85 millimeters, or as the mean 57.86 millimeters. Taking, then, the mean of these two results, we get 57.79 millimeters as the value of the distance between the centres of the external planes. Hence, if we assume the first of the two values obtained for the distance of the summits, 76.79 millimeters, we find

$$h-f=\frac{76.79-57.79}{2}=9.50 \text{ millimeters};$$

and if from the second result, 77.02 millimeters, we find

$$h-f=\frac{77.02-57.79}{2}=9.61 \text{ millimeters}.$$

These two elevations evidently differ but little from 9.41 millimeters, the altitude deduced from theory, (§ 40;) in the first case the difference does not amount to the $\frac{1}{100}$th part of this theoretical value, and in the second it hardly exceeds $\frac{2}{100}$ths. These differences undoubtedly arise from slight remains of heterogeneity in the liquids; it is probable that in the first case neither of the two liquids was absolutely homogeneous, and that the two contrary effects which thence resulted (§ 41) partly neutralized each other, whilst in the second case, the alcoholic liquid being rendered perfectly homogeneous, the effect of the slight heterogeneity of the oil exerted its full influence. However this may be, these differences in each case are so small that we may consider experiment as in accordance with theory, of which it evidently presents a very remarkable confirmation.

43. Mathematically considered, a cylindrical surface extends indefinitely in the direction of the axis of revolution. Hence it follows that the cylinder

included between the two rings constitutes one portion only of the complete figure of equilibrium. Hence also, if the liquid mass were free, it could not assume the cylindrical form as the figure of equilibrium; for the volume of this mass being limited, it would be necessary that the cylinder should be terminated on both sides by portions of the surface presenting other curvatures, which would not admit of the law of continuity. But this heterogeneity of curvature, which is impossible when the mass is free, becomes realizable, as our experiments show, through the medium of solid rings. As each of these renders the curvatures of the portions of the surface resting upon it (§ 20) independent of each other, the surface comprised between the two rings may then be of cylindrical curvature, whilst the two bases of the figure may present spherical curvatures. We therefore arrive at the very remarkable result, that with a liquid mass of a limited volume we may obtain isolated portions of figures of equilibrium, which in their complete state would be extended indefinitely.

44. With the view of obtaining a cylinder in which the proportion between the height and the diameter was still greater than that in Fig. 23, I replaced the rings previously employed by two others, the diameter of which was only 2 centimeters. I first tried to make a cylinder 6 centimeters in height, *i. e.*, the height of which was thrice the diameter; and in this operation I adopted a slightly different process from that of paragraph 38. The uniformity in the density of the two liquids being accurately established, I first gave the mass of oil a somewhat larger volume than that which the cylinder would contain; having then attached the mass to the two rings, I elevated the upper ring until it was at a distance of 6 centimeters from the other; this distance was measured by a scale introduced into the vessel and kept in a vertical position by the side of the liquid figure. In consequence of the excess of oil, the meridional line of the figure was convex externally; and as there was still a slight difference between the densities, this convexity was not symmetrical in regard to the two rings. I corrected this irregularity by successive additions of pure alcohol and alcohol of 16°, an operation which requires great circumspection, and towards the end of which these liquids could only be added in single drops. The figure being at last perfectly symmetrical, I carefully removed the excess of oil by applying the point of the syringe to a point at the equator of the mass, and in this manner I obtained a perfect cylinder. Subsequently, after having added some oil to the mass, I increased the distance between the rings until it was equal to 8 centimeters, *i. e.*, to four times their diameter. The oil was in sufficient quantity to allow of the meridional line of the figure being convex externally; but the curvature was not perfectly symmetrical, and I encountered still greater difficulties in regulating it than in the preceding case. The defect in the symmetry being ultimately corrected, the meridional convexity presented a versed sine of about 3 millimeters, (Fig. 25.) I then proceeded to the removal of the excess of oil; but before the versed sine was reduced to 2 millimeters, the figure appeared to have a tendency to become thin at its lower part and to swell out at the upper part, as if the oil had suddenly become slightly increased in density. At this moment I withdrew the syringe, so as to be enabled to observe the effect in question better; the change in form then became more and more pronounced; the lower part of the figure soon presented a true strangulation, the neck of which was situated nearly at a fourth part of the distance between the rings, (Fig. 26;) the constricted portion continued to narrow gradually, whilst the upper part of the figure became swollen; finally, the liquid separated into two unequal masses, which remained respectively adherent to the two rings; the upper mass formed a complete sphere, and the lower mass a doubly convex lens. The whole of these phenomena lasted a very short time only.

Fig. 25.

Fig. 26.

With a view to determine whether any particular cause had in reality produced the alteration of the densities, I approximated the rings; then, after having reunited the two liquid masses, I again carefully raised the upper ring, ceasing at the height of 7½ centimeters, so that the versed sine of the meridional convexity was slightly greater than when this was 8 centimeters. The figure was then found to be perfectly symmetrical, and it did not exhibit any tendency to deformity; whence it follows that the uniformity in the densities had not experienced any appreciable alteration. I recommenced, with still more care, the experiment with that figure which was 8 centimeters in height; and I was enabled to approach the cylindrical form still more nearly; but before it was attained, the same phenomena again presented themselves, except that the alteration in form was effected in an inverted manner, *i. e.*, the figure became narrow at the upper part and dilated at the base; so that after the separation into two masses, the perfect sphere existed in the lower ring and the lens in the upper ring. On subsequently uniting, as before, the two masses, and placing the rings at a distance of 7½ centimeters apart, the figure was again obtained in a regular and permanent form. Thus when we try to obtain between two solid rings a liquid cylinder the height of which is four times the diameter, the figure always breaks up spontaneously, without any apparent cause, even before it has attained the exactly cylindrical form. Now as the cylinder is necessarily a figure of equilibrium, whatever may be the proportion of the height to the diameter, we must conclude that the equilibrium of a cylinder the height of which is four times the diameter is unstable. As the shorter cylinders which I had obtained did not present analogous effects, I was anxious to satisfy myself whether the cylinders were really stable. I therefore again formed a cylinder 6 centimeters in height with the same rings; but this, when left to itself for a full half hour, presented a trace only of alteration in form, and this trace appeared about a quarter of an hour after the formation of the cylinder, and did not subsequently increase, which shows that it was due to some slight accidental cause.

The above facts lead us then to the following conclusions: 1st, that the cylinder constitutes a figure the equilibrium of which is stable when the proportion between its height and its diameter is equal to 3, and with still greater reason when this proportion is less than 3; 2d, the cylinder constitutes a figure the equilibrium of which is unstable when the proportion of its height to its diameter is equal to 4, and with still greater reason when it exceeds 4; 3d, consequently there exists an intermediate relation, which corresponds to the passage from stability to instability. We shall denominate this latter proportion *the limit of the stability of the cylinder.*

45. These conclusions, however, are liable to a well-founded objection. Our liquid figure is complex, because its entire surface is composed of a cylindrical portion and of two portions which present a spherical curvature. Now we cannot affirm that these latter portions exert no influence upon the stability or the instability of the intermediate portion, and consequently upon the value of the proportion which constitutes the limit between these two states. To allow of the preceding conclusions being rigorously applicable to the cylinder, it would be requisite that the figure should present no other free surface than the cylindrical surface, which is easily managed by replacing the rings by entire disks. I effected this substitution by employing disks of the same diameter as the preceding rings, but the results were not changed; the cylinder, 6 centimeters in height, was well formed, and was found to be stable; whilst the figure 8 centimeters in height began to change before becoming perfectly cylindrical, and was rapidly destroyed. The final result of this destruction did not, however, consist, as in the case of the rings, of a perfect sphere and a double convex lens, but, as evidently ought to have been the case, of two unequal portions of spheres,

respectively adherent to the two opposite solid surfaces. The limit of the stability of the cylinder, therefore, really lies between 3 and 4.

The experiments which we have just related are very delicate, and require some skill. In this, as in all other cases of measurements, the oil must be allowed to remain in the alcoholic mixture for two or three days, then the pellicle must be removed from it, (note to p. 254;) afterwards, when the mass, after having been again introduced into the vessel, has been attached to the two solid disks, some time must be allowed to elapse in order that the two liquids may be exactly at the same temperature; moreover, it must be understood that the experiments should be made in an apartment the temperature of which remains as constant as possible. Lastly, it is scarcely necessary to add, that when the alcoholic liquid is mixed, after having added small quantities of pure alcohol or alcohol at 16°, the movements of the spatula should be very slow, so as to avoid the communication of too much agitation to the mass of oil; we are even sometimes compelled momentarily to depress the upper disk, so as to give greater stability to the mass, and thus to prevent the movements in question from producing the disunion.

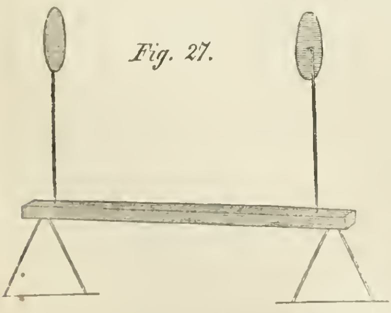
46. It might be asked whether the want of symmetry, which is constantly seen in the spontaneous modification of the above unstable figures, is the result of a law which governs these figures; or whether it simply arises, as we should be led to believe at first sight, from imperceptible differences still existing between the densities of the two liquids, which differences acting upon unstable figures might produce this want of symmetry, notwithstanding their extreme minuteness

After having concluded the preceding experiments, I imagined that to solve the question in point, all that would be requisite would be to arrange matters so that the axis of the figure, instead of being vertical, as in the above experiments, should have a horizontal direction. In fact, in the latter case, the slightest difference between the densities ought to have the effect of slightly curving the figure, but evidently cannot give the liquid any tendency to move in greater quantity towards one extremity of the figure than the other; whence it follows, that if the spontaneous alteration of the figure still occurs unsymmetrically, this can only be owing to a peculiar law.

On the other hand, if the figure really tends of itself to change its form unsymmetrically, it is clear that, in the case of the vertical position of the axis, the effect of a trace of difference between the densities ought to concur with that of the instability, and thus to accelerate the moment at which the figure commences to alter spontaneously. Consequently, on avoiding this extraneous cause by the horizontal direction of the axis of the figure, we may hope to approximate more nearly to the cylindrical form, or even to attain it exactly; we can, moreover, understand that the difficulty in the operations will be found to be considerably diminished.

I therefore constructed a solid system, presenting two vertical disks of the same diameter, placed parallel with each other, at the same height, and opposite each other. Each of these disks is supported by an iron wire fixed normally to its centre, then bent vertically downwards, and the lower extremities of these two wires are attached to a horizontal axis furnished with four small feet. This system is represented in perspective in Fig. 27. The diameter of the disks is 30 millimeters, but the distance which separates them is not four times this diameter. I thought that by approximating the figure more to the limit of stability, the operations

Fig. 27.



would require still less trouble; the distance in question is only 108 millimeters, so that the relation between the length and the diameter of the liquid cylinder which would extend between the two disks would be equal to 3.6.

We shall now detail the results obtained by the employment of this system. In the first place, the operations were much more easily performed.* In the second place, the figure still had a tendency to deformity before it had been rendered perfectly cylindrical; but this tendency always exhibited itself unsymmetrically, as in the vertical figures; from which circumstance alone we might conclude that the unsymmetrical nature of the phenomenon is not occasioned by a difference between the densities of the two liquids. In the third place, by a little management, I have pursued the experiment further, and succeeded in forming an exact cylinder.† This lasted for a moment; it then began to be narrowed at one part of its length, becoming dilated at the other, like the vertical figures; and the phenomenon of disunion was completed in the same manner, giving rise ultimately to two masses of different volumes.

I repeated the experiment several times, and always with the same results, except that the separation occurred sometimes on one, sometimes on the other side of the middle of the length of the figure. However, although the phenomenon is produced in an unsymmetrical manner with regard to the middle of the length of the figure, whether horizontal or vertical, on the contrary there is always symmetry with regard to the axis; in other words, throughout the duration of the phenomenon the figure remains constantly a figure of revolution. We may add here, that in the horizontal figure the respective lengths of the constricted and dilated portions appear to be equal; we shall show, in the following series, that this equality is rigorously exact, at least at the commencement of the phenomenon.

It is now evident that the alteration in the form of these cylinders is really the result of a property which is inherent in them. We shall hereafter deduce this property as a necessary consequence of the laws which govern a more general phenomenon.

It moreover results from the above experiment that the proportion 3.6 is still greater than the limit of stability, so that the exact value of the latter must lie between the numbers 3 and 3.6. It is obvious that this method of experiment might be employed to obtain a closely approximative determination of the value in question; I propose doing this hereafter, and I shall give an account of the result in the following series, when I shall have to return to the question of the limit of stability of the cylinder.

47. In the unstable cylinders which we have just formed, the proportion of the length to the diameter was inconsiderable; but what would be the case if we were to obtain cylinders of great length relatively to their diameter? Now, under certain circumstances, figures of this kind, more or less exactly cylindrical, may be realized, and we shall proceed to see what the results of the spontaneous rupture of equilibrium are.

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* The two disks in this solid system being placed at an invariable distance from each other, it is necessary, in making a mass of oil, the volume of which is not too great, adhere to them, to employ an extra piece consisting of a ring of iron wire of the same diameter as the disks, supported by a straight wire of the same metal, the free extremity of which is held in the hand. By means of this ring the mass, which has been previously attached to one of the disks, is drawn out until it is equally attached to the other; the ring is then removed. The latter removes a small portion of the mass at the same time; but on leaving the vessel it leaves this portion in the alcoholic liquid. It is then removed by means of the syringe.

† To effect this the following proceeding must be adopted for the removal of the excess of oil. The operation is at first carried on with a suitable rapidity until the figure begins to alter in form; the end of the point of the syringe is then drawn gently along the upper part of the mass, proceeding from the thickest to the other portion. This slight action is sufficient to move a minute quantity of oil towards the latter, and thus to re-establish the symmetry of the figure; a new absorption is then made, the figure again regulated, and these proceedings are continued until the exactly cylindrical form is attained.

A fact which I described in paragraph 20 of the preceding memoir, and which I shall now describe more in detail, affords us the means of obtaining a cylinder of this kind, and of observing its spontaneous destruction. When some oil is introduced by means of a small funnel into an alcoholic mixture containing a slight excess of alcohol, and the oil is poured in sufficiently quick to keep the funnel full, the liquid forms, between the point of the funnel and the bottom of the vessel where the mass collects, a long train, the diameter of which continues to increase slightly from the upper to the lower part, so as to form a kind of very elongated cone, which does not differ much from a cylinder.* This nearly cylindrical figure, the height of which is considerable in proportion to the diameter, remains without undergoing any perceptible alteration so long as the oil of which it consists has sufficient rapidity of transference; but when the oil is no longer poured into the funnel, and consequently the motion of transference is retarded, the cylinder is soon seen to resolve itself rapidly into a series of spheres, which are perfectly equal in diameter, equally distributed, and with their centres arranged upon the right line forming the axis of the cylinder.

To obtain perfect success, the elements of the experiment should be in certain proportions. The orifice of the funnel which I used was about 3 millimeters in diameter, and 11 centimeters in height. It rested upon the neck of a large bottle containing the alcoholic mixture, and its orifice was plunged a few millimeters only beneath the surface of the liquid. Lastly, the length of the cylinder of oil, or the distance between the orifice and the lower mass, was nearly 20 centimeters. Under these circumstances, three spheres were constantly formed, the upper of which remained adherent to the point of the funnel; the latter was therefore incomplete. We may add, that the excess of alcohol contained in the mixture should neither be too great nor too small; the proper quantity is found by means of a few preliminary trials.

48. The constancy and regularity of the result of this experiment complete then the proof that the phenomena to which the spontaneous rupture of equilibrium of an unstable liquid cylinder gives rise are governed by determinate laws.

In this same experiment, the transformation ensues too rapidly to allow of its phases being well observed; but the phenomena presented to us by larger and less elongated cylinders, *i. e.*, the formation of a dilatation and constriction in juxtaposition, and equal or nearly so in length, the gradual increase in thickness of the dilated portion and the simultaneous narrowing of the constricted portion, &c., authorize us to conclude that in the case of a cylinder the length of which is considerable in proportion to the diameter, the following order of things takes place: the figure becomes at first so modified as to present a regular and uniform succession of dilated portions, separated by constricted portions of the same length as the former, or nearly so. This alteration, the indications of which are very slight, gradually becomes more and more marked, the constricted portions gradually becoming narrower, whilst the dilated portions increase in thickness, the figure remaining a figure of revolution; at last the constrictions break, and each of the various parts of the figure, which are thus completely isolated from each other, acquire the spherical form. We must add, that the termination of the phenomenon is accompanied by a remarkable peculiarity, of which we have not yet spoken; but as it only constitutes, so to speak, an accessory portion of the general phenomenon, we shall transfer the description of it to a subsequent part of this memoir, (see § 62.)

49. It might be asked why, in the experiment which we have last described, the cylinder is only resolved into spheres when the rapidity of the transference of liquid of which it is composed is diminished. In fact, we cannot understand how a motion of transference could give stability to a liquid figure which in a

* The slight increase in diameter depends upon the retardation which the resistance of the surrounding liquid occasions in the movement of the oil.

state of repose was unstable. In explaining this apparent peculiarity, we must remark that, as the spontaneous transformation of an unstable cylinder is effected under the action of continued forces, the rapidity with which the phenomenon occurs ought to be accelerated; this may be, moreover, easily verified in experiments relating to larger and less elongated cylinders; this same rapidity ought, therefore, always to be very minute at the commencement of the phenomenon. Now, in the case in question, as the changes in figure occur in the liquid of the cylinder whilst this liquid is animated by a movement of transference, it is evident, from what we have stated, that if this movement of transference is sufficiently rapid, the changes of form could only acquire a very slightly marked development during the passage of the point of the funnel to the mass accumulated at the bottom of the vessel; so that, the liquid being continually renewed, there will be no time for any alteration in form to become very perceptible to the eye. Hence, so long as the rapidity of the flow is sufficiently great, the liquid figure will appear to retain its almost cylindrical form, although its length is considerable in comparison with its diameter. On the other hand, when the velocity of the transference is sufficiently small, there will be time for the alterations in form to take place in a perfect manner, and we shall be able to see the cylinder resolve itself into spheres throughout the whole of its length.

50. We shall now describe another method of experimenting, which allows us to observe the result of the transformation under less restrained and more regular conditions in some respects than those of the preceding experiment, and which will, moreover, lead us to new consequences as regards the laws of the phenomenon. We shall first succinctly describe the apparatus and the operations, and afterwards add the necessary details.

The principal parts of which the apparatus consists are: 1st, a rectangular plate of plate-glass, 25 centimeters in length, and 20 in breadth; 2d, two strips of the same glass, 13 centimeters in length, and $\frac{5}{6}$ millimeters in thickness, perfectly prepared and polished at the edges; 3d, two ends of copper wire, about 1 millimeter in thickness, and 5 centimeters in length; these wires should be perfectly straight, and one extremity of each of them should be cut very accurately, then carefully amalgamated. The plate being placed horizontally, the two strips are laid flat upon its surface and parallel with its long sides, so as to leave an interval of about a centimeter between them; the two copper wires are then introduced into this, placing them in a right line in the direction of the length of the strips, and in such a manner that the amalgamated extremities are opposite to, and a few centimeters distant from, each other. A globule of very pure mercury, from 5 to 6 centimeters in diameter, is next placed between the same extremities; the two strips of glass are then approximated until they touch the wires, so as only to leave between them an interval equal in width to the diameter of these wires. The little mass of mercury, being thus compressed laterally, necessarily becomes elongated, and extends on both sides towards the amalgamated surfaces. If it does not reach them, the wires are made to slide towards them until contact and adhesion are established. The wires are then moved in opposite directions, so as to separate them from each other, which again produces elongation of the little liquid mass and diminution of its vertical dimensions. By proceeding carefully, and accompanying the operation with slight blows given with the finger upon the apparatus to facilitate the movements of the mercury, we succeed in extending the little mass until its vertical thickness is everywhere equal to its horizontal thickness, *i. e.*, to that of the copper wires. Thus the mercury forms a liquid wire of the same diameter as the solid wires to which it is attached, and from 8 to 10 centimeters in length. This wire, considering the small size of its diameter, which renders the action of gravitation insensible in comparison with that of molecular attraction, may be considered as exactly cylindrical; so that in this manner we obtain a liquid cylinder, the length of which is from 80 to 100 times its diame-

ter, and attached by its extremities to solid parts, which cylinder preserves its form so long as it remains imprisoned between the strips of glass. Weights are next placed upon the parts of the two copper wires which project beyond the extremities of the bands, so as to maintain these wires in firm positions; lastly, by means which we shall point out presently, the two strips of glass are raised vertically. At the same instant, the liquid cylinder, being liberated from its shackles, becomes transformed into a numerous series of isolated spheres, arranged in a straight line in the direction of the cylinder from which they originated.* Ordinarily the regularity of the system of spheres thus obtained is not perfect; the spheres present differences in their respective diameters and in the distances which separate them; this undoubtedly arises from slight accidental causes, dependent upon the method of operation; but the differences are sometimes so small that the regularity may be considered as perfect. As regards the number of spheres corresponding to a cylinder of determinate length, it varies in different experiments; but these variations, which are also due to slight accidental causes, are comprised within very small limits.

51. Let us now complete the description of the apparatus, and add some details regarding the operations. As the plate of glass requires to be placed in a perfectly horizontal position, it is supported for this purpose upon four feet with screws. A small transverse strip of thin paper is glued to each of the extremities of the lower surface of the strips of glass, in such a manner that the strips of glass resting upon the plate through the medium of these small pieces of paper, their lower surface is not in contact with the surface of the plate. Without this precaution, the strips of glass might contract a certain adhesion to the plate, which would introduce an obstacle when the strips are raised vertically. Moreover, the latter are furnished, on their upper surface and at a distance of 6 millimeters from each of their extremities, with a small screw placed vertically in the glass with the point upwards, firmly fixed to it with mastic, and rising 8 millimeters above its surface. These four screws are for the purpose of receiving the nuts which fix the strips to the system by means of which they are elevated. This system is made of iron; it consists, in the first place, of two rectangular plates, 55 millimeters in length, 12 in breadth, and 3 in thickness. Each of them is pierced, perpendicularly to its large surfaces, by two holes, so situated, that on placing each of these plates transversely upon the extremities of the two strips of glass, the screws with which the latter are furnished fit into these four holes. The screws being long enough to project above the holes, nuts may then be adapted to them, so that on screwing them the strips of glass become fixed in an invariable position with regard to each other. The holes are of an elongated form in the direction of the length of the iron plates; hence, after having loosened the nuts, the distance between the two strips of glass may be increased or diminished without the necessity of removing the plates. A vertical axis, 5 centimeters in height, is implanted upon the middle of the upper surface of each of the plates; and the upper extremities of these two axes are connected by a horizontal axis, at the middle of which a third vertical axis commences; this is directed upwards, and is 15 centimeters in length. The section of the latter axis is square, and it is 5 millimeters in thickness. When the nuts are screwed up, it is evident that the strips of glass, the iron plates, and the kind of fork which connects them, constitute an invariable system. The long vertical axis serves to direct the movement of this system; with this view, it passes with very slight friction through an aperture of the same section as itself, and 5 centimeters in length, pierced in a piece which is fixed very firmly by a suitable support 10 centimeters above the plate of glass. Lastly, the perforated piece is provided laterally with a thumb-screw, which allows the axis to be screwed

* We may remark that the conversion of a metallic wire into globules by the electric discharge must undoubtedly be referred to the same order of phenomena.

into the tube. By this arrangement, if all parts of the apparatus have been carefully finished, when once the little nuts have been screwed up, the two strips of glass can only move simultaneously in a parallel direction to each other, and always identically in the same direction perpendicular to the plate of glass When the liquid cylinder is well formed, and the weights are placed upon the free portions of the copper wires, the finger is passed under the horizontal branch of the fork, and the movable system is raised to a suitable distance above the plate of glass; it is then maintained at this height by means of the thumb-screw, so as to allow the result of the transformation of the cylinder to be observed. As the amalgamation of the copper wires always extends slightly upon their convex surface, the latter is coated with varnish, so that the amalgamation only occurs upon the small plane section. It would be almost impossible to judge by simple inspection of the exact point at which the separation of the copper wires from each other, to allow of the liquid attaining a cylindrical form, should be discontinued. To avoid this difficulty, the length of the cylinder is given beforehand, and this length is marked by two faint scratches upon the lateral surface of one of the strips of glass; the weight of the globule of mercury, which is to form a cylinder of this diameter and of the length required, is then determined by calculation from the known diameter of the wire; lastly, by means of a delicate balance, the globule to be used in the experiment is made exactly of this weight. All that then remains to be done is to extend the little mass until the extremities of the copper wires between which it is included have reached the marks traced upon the glass. Lastly, in making a series of experiments, the same mercury may be used several times if the isolated spheres are united into a single mass at the end of each observation. However, after a certain number of experiments, the mercury appears to lose its fluidity, and the mass always becomes disunited at some point, in spite of all possible precautions, before it has become extended to the desired length, which phenomena arise from the solid wires imparting a small quantity of copper to the mercury. The latter must then be removed, the plates of glass and the strips cleaned, and a new globule taken. The amalgamation of the wires also sometimes requires to be renewed.

52. By means of the above apparatus and methods, I have made a series of experiments upon the transformation of the cylinders; but before relating the results, it is requisite for their interpretation that we should examine the phenomenon a little more closely.

Let us imagine a liquid cylinder of considerable length in proportion to its diameter, and attached by its extremities to two solid bases; let us suppose that it is effecting its transformation, and let us consider the figure at a period of the phenomenon anterior to the separation of the masses, *i. e.*, when this figure is still composed of dilatations alternating with constrictions. As the surfaces of the dilatations project externally from the primitive cylindrical surface, and those of the constrictions on the contrary are internal to this same surface, we can imagine in the figure a series of plane sections perpendicular to the axis, and all having a diameter equal to that of the cylinder; these sections will evidently constitute the limits which separate the dilated from the constricted portion, so that each portion, whether constricted or dilated, will be terminated by two of them; moreover, as the two solid bases are necessarily part of the sections in question, each of these bases should occupy the very extremity of a constricted or dilated portion. This being granted, three hypotheses present themselves in regard to these two portions of the figure, *i. e.*, to those which rest respectively upon each of the solid bases. In the first place, we may suppose that both of the portions are expanded. In this case each of the constrictions will transfer the liquid which it loses to the two dilatations immediately adjacent to it; the movements of transport of the liquid will take place in the same manner throughout the whole extent of the figure, and the

transformation will take place with perfect regularity, giving rise to isolated spheres exactly equal in diameter, and at equal distances apart. This regularity will not, however, extend to the two extreme dilatations; for as each of these is terminated on one side by a solid surface, it will only receive liquid from the constriction which is situated on the other side, and will, therefore, acquire less development than the intermediate dilatations. Under these circumstances, then, after the termination of the phenomenon, we ought to find two portions of spheres respectively adherent to two solid bases, each presenting a slightly less diameter than that of the isolated spheres arranged between them.

In the second place, we may admit that the terminal portions of the figure are, one a constriction and the other a dilatation. The liquid lost by the first, not being then able to traverse the solid base, will necessarily all be driven into the adjacent dilatation; so that, as the latter receives all the liquid necessary to its development on one side only, it will receive none from the opposite side; consequently all the liquid lost by the second constriction will flow in the same manner into the second dilatation, and so on up to the last dilatation. The distribution of the movements of transport will, therefore, still be regular throughout the figure, and the transformation will ensue in a perfectly regular manner. This regularity will evidently extend even to the two terminal portions, at least so long as the constrictions have not attained their greatest depth; but beyond that point this will not exactly be the case, for independence being then established between the masses, each of the dilatations, excepting that which rests upon the solid base, will enlarge simultaneously on both sides, so as to pass into the condition of the isolated sphere, by appropriating to itself the two adjacent semi-constrictions, whilst the extreme dilatation can enlarge on one side. Consequently, after the termination of the phenomenon, we should find, at one of the soild bases, a portion of a sphere of but little less diameter than that of the isolated spheres, and at the other base a much smaller portion of a sphere, arising from the semi-constriction which has remained attached to it.

Lastly, in the third place, let us suppose that the terminal portions of the figure were both constrictions, in which case, after the termination of the phenomenon, a portion of a sphere equal to the smallest of the two above would be left to each of the solid bases. In this case, to be more definite, let us start from one of these terminal constrictions; for instance, that of the left. All the liquid lost by this first constriction being driven into the contiguous dilatation, and being sufficient for its development, let us admit that all the liquid lost by the second constriction also passes into the second dilatation, and so on; then all the dilatations, excepting the last on the right, will simply acquire their normal development; but the right dilatation, which, like each of the others, receives from that part of the constriction which precedes it the quantity of liquid necessary for its development, receives in addition the same quantity of liquid from that part of the constriction which is applied to the adjacent solid, so that it will be more voluminous than the others. Hence it is evident, in the case in point, that the opposed actions of the two terminal constrictions introduce an excess of liquid into the rest of the figure. Now, whatever other hypothesis may be made respecting the distribution of the movements of transport, it must always happen either that the excess of volume is simultaneously distributed over all the dilatations, or that it only augments the dimensions of one or two of them; but the former of these suppositions is evidently inadmissible, on account of the complication which it would require in the movements of transport; hence we must admit the second, and then the isolated spheres will not all be equal. Thus this third mode of transformation would necessarily of itself induce a cause of irregularity; and, moreover, it would not allow of a uniform distribution of the movements of transport, be-

cause there would be opposition in regard to these movements, at least in the terminal constrictions.

It may, therefore, be regarded as very probable that the transformation takes place according to one or the other of the two first methods, and never according to the third, *i. e.*, that things will be so arranged that the figure which is transformed may have for its terminal portions either two dilatations, or one constriction and one dilatation, but not two constrictions. In the former case, as we have seen, the movement of the liquid of all the constrictions would ensue on both sides simultaneously; and in the second this movement would occur in all in one and the same direction. If this is really the natural arrangement of the phenomenon, we can also understand how it will be preserved even when it is disturbed in its regularity by slight extraneous causes. Now, this, as we shall see, is confirmed by the experiments relating to the mercurial cylinder. Although the transformation of this cylinder has rarely yielded a perfectly regular system of spheres, I have found in the great majority of the results either that each of the soild bases was occupied by a mass little less in diameter than the isolated spheres, or that one of the bases was occupied by a mass of this kind and the other by a much smaller mass.

53. For the sake of brevity, let us denominate *divisions* of the cylinder those portions of the figure each of which furnishes a sphere, whether we consider these portions in the imagination as in the cylinder itself, before the commencement of the transformation, or whether we take them during the accomplishment of the phenomenon, *i. e.*, during the modifications which they undergo in arriving at the spherical form. The length of a division is evidently that distance which, during the transformation, is comprised between the necks of two adjacent constrictions; consequently it is equal to the sum of the lengths of a dilatation and two semi-constrictions. Let us, therefore, see how the length in question, *i. e.*, that of a division, may be deduced from the result of an experiment. Let us suppose the transformation to be perfectly regular, and let λ be the length of a division, l that of the cylinder, and n the number of isolated spheres found after the termination of the phenomenon. Each of these spheres being furnished by a complete division, and each of the two terminal masses by part of a division, the length l will consist of n times λ, plus two fractions of λ. To estimate the values of these fractions, we must recollect that the length of a constriction is exactly or apparently equal to that of a dilatation, (§ 46;) now, in the first of the two normal cases, (§ 52,) *i. e.*, when the masses remaining adherent to the bases after the termination of the phenomenon are both of the large kind, each of them evidently arises from a dilatation plus half a constriction, therefore three-fourths of a division; the sum of the lengths of the two portions of the cylinder which have furnished these masses is, therefore, equal to once and a half λ, and we shall have in this case $l = (n + 1.5)\lambda$, whence $\lambda = \dfrac{l}{n + 1.5}$. In the second case, *i. e.*, when the terminal masses consist of one of the large and the other of the small kind, the latter arises from a semi-constriction, or a fourth of a division, so that the sum of the lengths of the portions of the cylinder corresponding to these two masses is equal to λ; consequently we shall have $\lambda = \dfrac{l}{n + 1}$.

As the respective denominators of these two expressions represent the number of divisions contained in the total length of the cylinder, it follows that this number will always be either simply a whole number, or a whole number and a half. On the other hand, as the phenomenon is governed by determinate laws, we can understand that for a cylinder of given diameter composed of a given liquid, and placed under given circumstances, there exists a normal length which the divisions tend to assume, and which they would rigorously assume if the total length of the cylinder were infinite. If, then, it happens

that the total length of the cylinder, although limited, is equal to the product of the normal length of the divisions by a whole number, or rather a whole number plus a half, nothing will prevent the divisions from exactly assuming this normal length. If, on the other hand, which is generally the case, the total length of the cylinder fulfils neither of the preceding conditions, we should think that the divisions would assume the nearest possible to the normal length; and then, all other things being equal, the difference will evidently be as much less as the divisions are more numerous, or, in other words, as the cylinder is longer. We should also believe that the transformation would adopt that of the two methods which is best adapted to diminish the difference in question, and this is also confirmed by experiment, as we shall see presently. Hence, although, as I have already stated, the transformation of the cylinder of mercury almost always ensues in one of the two normal methods, the result is rarely very regular; we must, therefore, admit that slight accidental disturbing causes in general render the divisions formed in any one experiment unequal in length; but then the expressions of λ obtained above evidently give in each experiment the mean length of these divisions, or, in other words, the common length which the divisions would have taken if the transformation had occurred in a perfectly regular manner, giving rise to the same number of isolated spheres and to the same state of the terminal masses.

Lastly, since the third method of transformation presents itself, *i. e.*, since it sometimes happens that each of the bases is occupied by a mass of the small kind, if we would leave out of consideration the particular cause of irregularity inherent in this method, (the preceding paragraph,) and find the corresponding expression of λ, it need only be remarked that each of the terminal masses then proceeds from a semi-constriction or the fourth of a division, which will evidently give $\lambda = \frac{l}{n + 0.5}$.

54. I shall now relate the results of the experiments. The diameter of the copper wires, consequently of the cylinder, was 1.05 millimeter. I first gave the cylinder a length of 90 millimeters, and repeated the experiment ten times, noting after each the number of isolated spheres produced, and the state of the masses adherent to the bases; I then calculated for each result the corresponding value of the length of a division, by means of that of the three formulæ of the preceding paragraph which refers to this same result, I afterwards made ten more experiments, giving the cylinder a length of 100 millimeters, and also calculated the corresponding values of the length of a division. The table contains the results furnished by these cylinders, and the values deduced for the length of a division. I only obtained a perfectly regular result in one case in each series; I have placed an * opposite the corresponding number of isolated spheres.

Length of the cylinder 90 millimeters.			Length of the cylinder 100 millimeters.		
Number of isolated spheres.	Masses adherent to the bases.	Length of a division.	Number of isolated spheres.	Masses adherent to the bases.	Length of a division.
		millims.			*millims.*
10	Two large	7.83	11	One large and one small	8.33
*12	Two large	6.67	14	Two large	6.45
12	Two small	7.20	14	Two large	6.45
15	Two large	5.45	14	Two large	6.45
14	Two large	5.81	*14	One large and one small	6.67
11	Two large	7.20	13	One large and one small	7.14
11	Two large	7.20	11	Two large	8.00
12	One large and one small	6.92	14	One large and one small	6.67
13	Two large	6.21	13	Two large	6.90
11	Two large	7.20	10	Two large	8.69

This table shows, in the first place, that the different values obtained for the length of a division are not so far removed from each other as to prevent our perceiving a constant value, the uniformity of which is only altered by the influence of slight accidental causes. In the second place, out of twenty experiments, it happened once only that the masses adherent to the bases were both of the small kind. In the third place, both the perfectly regular results have given identically the same value for the length of a division; this value, expressed approximatively to two decimal places, is 6.67 millimeters; but its exact expression is $6\frac{2}{3}$ millimeters; for the operation to be effected consists, in the case of the first series, in the division of 90 millimeters by 13.5, and, in the case of the second series, in the division of 100 millimeters by 15. As the two lengths given to the cylinder are considerable in proportion to the diameter, and consequently the numbers of division are tolerably large, this value, $6\frac{2}{3}$ millimeters, ought very nearly, if not exactly, to constitute that of the normal length of the divisions. It is seen, moreover, that to give the divisions this closely approximative or exact value of the normal length, the transformation has chosen, in one case the first, in the other case the second method.

55. Let us pursue our inquiry into the laws of the phenomenon with which we are engaged; we shall soon make an important application of them, and it will then be understood why so extensive a development is given to this part of our work. It might be regarded as evident *à priori* that two cylinders formed of the same liquid and placed in the same circumstances, but differing in diameter, would tend to become divided in the same manner, *i. e.*, that the respective normal lengths of the divisions would be to each other in the proportion of the diameters of these cylinders.

In order to verify this law by experiment, I procured some copper wires, the diameter of which was exactly double that of the first, therefore equal to 2.1 millimeters, and I made with them a new series of ten experiments, giving the cylinder a length of 100 millimeters. This series also furnished me with only a single perfectly regular result, which I have denoted as before by an * placed opposite the corresponding number of isolated spheres. The following is the table relating to this series:

Number of isolated spheres.	Masses adherent to the bases.	Length of a division.
		millims.
7	Two small	13.33
6	Two large	13.33
6	One large and one small	14.28
7	One large and one small	12.50
*6	Two large	13.33
6	Two large	13.33
6	One large and one small	14.28
8	One large and one small	11.11
8	Two small	11.76
6	One large and one small	14.28

By stopping at the second decimal place, we have, as is evident, 13.33 millimeters for the value of the length of a division corresponding to the perfectly regular result; but as the operation which yields it consists in the division of 100 by 7.5, the value when perfectly expressed is $13\frac{1}{3}$ millimeters. This then is very nearly, if not exactly, the normal length of the divisions of this new cylinder; now this length, $13\frac{1}{3}$ millimeters, is exactly twice the length, $6\frac{2}{3}$ millimeters, which belongs to the divisions of the cylinder of the preceding

paragraph; these two lengths are therefore, in fact, in the proportion to each other of the diameters of the two cylinders.

As the perfectly regular result of the above table has given a mass of the larger kind to each base, it follows, that to enable the divisions of the cylinder itself to assume their normal length, or the nearest possible length to this, the transformation has necessarily ensued according to the former method; whilst in regard to a cylinder the diameter of which is a half less, and the total length of which is the same, 100 millimeters, the transformation ensued according to the second method, (§ 54.)

Here, also, the case in which there are two masses of the small kind to the solid bases is the least frequent, although it occurred twice. Lastly, the different values of the length of a division are more concordant than in the second series relating to the first diameter, and consequently show the tendency towards a constant value better; we also see that the normal length is that which is most frequently reproduced.

56. According to the law which we have just established, when the nature of the liquid and external circumstances do not change, the normal length of the divisions is proportional to the diameter of the cylinder; or, in other words, the proportion of the normal length of the divisions to the diameter of the cylinder is constant.

As we have seen, the diameter of the cylinder in paragraph 54 was 1.05 millimeters, and the normal length of its divisions was very little less than 6.67 millimeters; consequently, when the liquid used is mercury and the cylinder rests upon a plate of glass, the value of the constant proportion in question is $\frac{6.67}{1.05} = 6.35$, which approximates closely.

To ascertain whether the nature of the liquid and external circumstances exert any influence upon this proportion, we shall now determine the value of the latter in the case of a cylinder of oil formed in the alcoholic mixture, which may be effected, at least approximatively, with the aid of the result of the experiment in paragraph 47. To simplify the considerations, we shall suppose that the transformation does not commence until the rapidity of transference has entirely ceased. The point of the funnel, on the one hand, and the section by which the imperfect liquid cylinder is in contact with the mass which collects at the bottom of the vessel, on the other hand, may then be regarded as playing the part of the two bases of the figure. Now it is evident that, as regards the second of these bases, the last portion of the figure which is transformed should be a constriction; for if it constituted a dilatation, there would be discontinuity of the curvature at the junction of the respective surfaces of the latter and the large mass, which is inadmissible. But the same reason does not apply to the other base; and experiment shows that in this case a dilatation is formed, because after the termination of the phenomenon we always find at the point of the funnel a mass comparable to the isolated spheres. Hence in this experiment the transformation ensues according to the second method. Therefore, as the whole length of the figure is about 200 millimeters, and as the transformation constantly yields two isolated spheres, the mean length of the divisions has (§ 53) for its approximative value $\frac{200}{3}$ millimeters $= 66.7$ millimeters; I say the mean length, because, as the diameter of the figure increases slightly from the summit towards the base, the divisions are probably not exactly equal in length. It must be added here, that the transformation ensues under circumstances which are always identical, and consequently, in the absence of accidental disturbing causes, the above quantity ought to represent the normal length of the divisions, or the nearest possible length to the latter. Now, I

estimate the mean diameter of the figure before the transformation at about 4 millimeters; we should consequently have $\frac{66.7}{4} = 16.7$ as the approximative value of the proportion between the normal length of the divisions and the diameter of the cylinder. This is, therefore, approximatively the constant proportion sought in the case of a cylinder of oil formed in the alcoholic mixture; now this proportion, as is evident, is much greater than that which belongs to the case of a cylinder of mercury resting upon a plate of glass.

In fact, the length 66.7 millimeters may differ somewhat materially from the normal length; for if, on the one hand, the whole length of the figure of oil is considerable in regard to its diameter, on the other hand, the number of divisions which form there is very small. Let us then see, for instance, what is the least value which the normal length of these divisions may have. We must in the first place remark, that in this case, notwithstanding the absence of disturbing causes, the third method of transformation is possible; in fact, as the lower constriction is adherent to a liquid base, nothing can prevent the oil which it loses from traversing this base to reach the large mass, so that in the third method, also, the direction of the movements of transport may be the same in regard to all the constrictions, (§ 52.) This granted, as the denominator of the expression which gives the length of one division can only vary by half units, (53,) and as the length which we have found resulted from the division of 200 millimeters by 3, it follows that the length immediately below would be $\frac{200}{3.5}$ millimeters $= 57.1$ millimeters, which would correspond to three isolated spheres and a transformation disposed according to the third method. But as matters do not take place in this manner, since there are never more than two isolated spheres formed, and the transformation always ensues according to the second method, we must conclude that the normal length of the divisions approximates more closely to the length found, 66.7 millimeters, than the length 57.1 millimeters. If, then, the normal length is greater than the first of these two quantities, it must at least be more than their mean, *i. e.*, 61.9 millimeters; consequently the relation of the normal length of the divisions and the diameter of the cylinder is necessarily greater than $\frac{61.9}{4} = 15.5$; now this latter number considerably exceeds the number 6.35, which corresponds to the mercurial cylinder.

Thus, the proportion of the normal length of the divisions to the diameter of the cylinder varies, sometimes according to the nature of the liquid, sometimes according to external circumstances, at others according to both these elements.

57. But I say that there is a limit below which this proportion cannot descend, and that this is exactly the limit of stability. Let us imagine a liquid cylinder of sufficient length in proportion to its diameter, comprised between two solid bases, and the transformation of which is taking place with perfect regularity. Suppose, for the sake of clearness, that the phenomenon ensues according to the second method, or, in other words, that the terminal portions of the figure consist one of a constriction, the other of a dilatation; then, as we have seen, (§ 52,) the regularity of the transformation will extend to these latter portions; *i. e.*, the terminal constriction and the dilatation will be respectively identical with the portions of the same kind of the rest of the figure. Let us then take the figure at that period of the phenomenon at which it still presents constrictions and dilatations, and let us again consider the sections, the diameter of which is equal to that of the cylinder, (§ 52.) Let us start from the terminal constricted portion; the solid base upon which this rests, and which constitutes the first of the sections in question, will occupy, as we have shown, the origin of the constric-

tion itself; we shall then have a second section at the origin of the first dilatation; a third at the origin of the second constriction; a fourth at the origin of the second dilatation, and so on; so that all the sections of the even series will occupy the origins of the dilatations, all those of the odd series the origins of the constrictions. The interval comprised between two consecutive sections of the odd series will therefore include a constriction and a dilatation; and as the figure begins with a constriction and terminates with a dilatation, it is clear that its entire length will be divided into a whole number of similar intervals. In consequence of the exact regularity which we have supposed to exist in the transformation, all the intervals in question will be equal in length; and as the moment at which we enter upon the consideration of the figure may be taken arbitrarily from the origin of the phenomenon to the maximum of the depth of the constrictions, it follows that the equality of length of the intervals subsists during the whole of this period, and that, consequently, the sections which terminate these intervals preserve during this period perfectly fixed positions. Besides the parts of the figure respectively contained in each of the intervals undergoing identically and simultaneously the same modifications, the volumes of all these parts remain equal to each other; and as their sum is always equal to the total volume of the liquid, it follows that, from the origin of the transformation to the maximum of depth of the constrictions, each of these partial volumes remains invariable, or, in other words, no portion of liquid passes from any one interval into the adjacent ones. Thus, at the instant at which we consider the figure, on the one hand, the two sections which terminate any one interval will have preserved their primitive positions and their diameters; and on the other hand these sections will not have been traversed by any portion of liquid. Matters will then have occurred in each interval in the same manner as if the two sections by which it is terminated had been solid disks. But the transformation cannot ensue between two solid disks, if the proportion of the distance which separates the disks to the diameter of the cylinder is less than the limit of stability; the proportion of the length of our intervals and the diameter of the cylinder cannot then be less than this limit. Now, the length of an interval is evidently equal to that of a division; for the first, in accordance with what we have seen above, is the sum of the lengths of a dilatation and a constriction; and the second is the sum of the lengths of a dilatation and two semi-constrictions, (§ 53;) the proportion of the length of a division to the diameter of the cylinder cannot then be less than the limit of stability; and we may remark here that this conclusion is equally true, whether the divisions are able or not to assume exactly their normal length.

58. Let us now attempt to ascertain the influence of the nature of the liquid and that of external circumstances, commencing with the latter. Our liquid cylinder of mercury, along the whole of the line at which it touches the plate of glass, must contract a slight adherence to this plate, which adherence must more or less impede the transformation. To discover whether this resistance exerted any influence upon the normal length of the divisions, consequently upon the proportion of the latter to the diameter of the cylinder, a simple means presented itself, viz., to augment this resistance. To arrive at this result, I arranged the apparatus in such a manner as to remove only one of the strips of glass, so that the liquid figure then remained simultaneously in co tact with the plate and the other strip. I again repeated the experiment ten times, using copper wires 1.05 millimeters in diameter, and giving the cylinder a length of 100 millimeters. The following were the results:

Number of isolated spheres.	Masses adherent to the bases.	Length of a division.
		millims.
9	One large and one small	10.00
8	One large and one small	11.11
9	One large and one small	10.00
8	One large and one small	11.11
11	Two small	8.69
8	One large and one small	11.11
8	One large and one small	11.11
8	Two large	10.53
8	One large and one small	11.11
6	Two large	13.33

It is evident that the different values of the length of a division, with a single exception, are all obviously greater than all those which relate to a cylinder of the same diameter, the surface of which only touches the glass by a single line, (§ 54.) We must thence conclude that, all other things being the same, the length of the divisions increases with the external resistance; consequently, under the action of the same resistance, this length is necessarily greater than it would be if the convex surface of the cylinder had been perfectly free.

In the above series neither of the results appears to be very regular; but we can readily understand that the mean of the values of the third column will approach the normal length of the divisions. This is, moreover, confirmed by the tables in §§ 54 and 55. If we take in the former the respective means of the values of the two series, we find for one 6.77 millimeters, and for the other 7.17 millimeters, quantities, the first of which is nearly equal to the length 6.67 millimeters, which may be considered as the normal length, and from which the second does not differ much; and if likewise we take the relative mean in the following table, we find 13.15 millimeters, a quantity very near the length 13.33 millimeters, which in the case of the second table may also be regarded as the normal length. Now, the corresponding mean in the above table is 10.81 millimeters; consequently, in the case of two lines of contact we have $\frac{10.81}{1.05} = 10.29$ as the approximate value of the proportion of the normal length of the divisions to the diameter of the cylinder, whilst in the case of a single line of contact we found only 6.35. Hence the proportion between the normal length of the divisions and the diameter of the cylinder increases by the effect of an external resistance.

59. Let us proceed to the influence of the nature of the liquid. All liquids are more or less viscid; *i. e.*, their molecules do not enjoy perfect mobility with regard to each other. Now, this gives rise to an internal resistance, which must also render the transformation less easy; and as external resistances increase the length of the divisions, we can understand that the viscidity will act in the same manner; consequently, all other things being equal, the proportion now under consideration will increase with this viscidity. But, on the other hand, with equal curvatures, the intensities of the forces which produce the transformation vary with the nature of the liquid; for these intensities depend, in the case of each liquid, upon that of the mutual attraction of the molecules. Now, it is clear that the viscidity will exert so much more influence upon the length of the divisions as the intensities of the forces in question are less. Thus, leaving external resistances out of the question, the proportions of the normal length of the divisions to the diameters will be greater in proportion to the viscidity of the liquid and the feebleness of the configuring forces.

The intensities of the configuring forces corresponding to different liquids may be compared numerically for the same curvatures. In fact, let us first bear in mind that the pressure corresponding to one element of the superficial layer, and reduced to unity of the surface, is expressed by (§ 4,)

$$P + \frac{A}{2}\left(\frac{1}{R} + \frac{1}{R'}\right).$$

Now, the value of the part P of this pressure being the same for all the elements of the superficial layer, and the pressures being transmitted throughout the mass, this part P will always be destroyed, whether equilibrium exists in the liquid figure or not; so that the active part of the pressure (that which constitutes the configuring force) will have for its measure simply $\frac{A}{2}\left(\frac{1}{R} + \frac{1}{R'}\right)$. Hence it is evident that when the curvatures are equal, the intensity of the configuring force arising from one element of the superficial layer is proportional to the coefficient A. Now, this coefficient is the same as that which enters into the known expression of the elevation or depression of a liquid in a capillary tube: consequently the measures relating to this elevation or depression will give us, in the case of each liquid, the value of the coefficient in question. Hence we may also say that the proportion of the normal length of the divisions to the diameter of the cylinder will be greater as the liquid is more viscid and as the value of A which corresponds to the latter diminishes. For instance, oil is much more viscid than mercury; on the other hand, it would be easy to show that the value of A is much less for the first than for the second of these two liquids; lastly, this value must be much diminished in regard to our figure of oil by the presence of the surrounding alcoholic liquid, the mutual attraction of the molecules of the two liquids in contact diminishing the intensities of the pressures, (§ 8.) This is why the proportion belonging to a cylinder of oil formed in the alcoholic mixture considerably exceeds that belonging to a cylinder of mercury resting upon a plate of glass, notwithstanding the slight external resistance to which the latter is subjected.

60. It follows from this discussion concerning the resistances that the smallest value which the proportion of the normal length of the divisions to the diameter of the cylinder could be supposed to have corresponds to that case in which there is simultaneously complete absence of external resistance and of viscidity; and, after the demonstration given in § 57, this least value will be at least equal to the limit of stability. Now, as all liquids are more or less viscid, it follows that, even on the hypothesis of the annihilation of all external resistance, the proportion in question will always exceed the limit of stability; and since this is more than 3, this proportion will, *à fortiori*, be always more than 3.

It is conceivable that the least value considered above, *i. e.*, that which the proportion would have in the case of complete absence of resistance, both internal as well as external, would be equal to the limit of stability itself, or would very slightly exceed it. In fact, on the one hand, the proportion approximates to this limit as the resistances diminish, and on the other hand, if it exceeds it, the transformation becomes possible, (§ 57;) hence we see no reason why it should differ sensibly from it if the resistances were absolutely null. The results of our experiments, moreover, tend to confirm this view. First, since the proportion belonging to our cylinder of mercury descends from 10.29 to 6.35, passing from that case in which the cylinder touches the glass at two lines to that where it touches it at a single one only, (§ 58,) it is clear that if this latter contact itself could be suppressed, which would leave the influence of the viscidity alone remaining, the proportion would become much less than

6.35; and as, on the other hand, it must exceed 3, we might admit that it would at least lie between the latter number and 4, so that it would closely approximate the limit of stability. If, then, it were possible to exclude the viscidity also, the new decrease which the proportion would then experience, would very probably bring the latter to the very limit in question, or at least to a value differing but exceedingly little from it. Thus, on the one hand, the least value of the proportion, that corresponding to the complete absence of resistances, would not differ, or scarcely so, from the limit of stability; and on the other hand, under the influence of viscidity alone, the proportion appertaining to the mercury would be but little removed from this least value. Hence it is evident that the influence of the viscidity of mercury is small, which is moreover explained by the well-known feebleness of this same viscidity.

We can now understand in the case of other but very slightly viscid liquids, such as water, alcohol, &c., where the viscidity is not able to form more than a minimum resistance, that this viscidity, notwithstanding the differences in the intensities of the configuring forces, will also exert only a feeble influence upon the proportion in question. Hence it results that, in the absence of all external resistance, the values of this proportion respectively corresponding to the various very slightly viscid liquids cannot be very far removed from the limit of stability; and as the smallest whole number above this is 4, we may in regard to these liquids adopt this number as representing the mean approximative probable value of the proportion in question.

Starting from this value, calculation gives us the number 1.82 as the proportion of the diameter of the isolated spheres which result from the transformation to the diameter of the cylinder, and the number 2.18 for the proportion between the distance of two adjacent spheres and this diameter.

61. There is another consequence arising from our discussion. For the sake of simplicity let the diameter of the cylinder be taken as unity. The proportion of the normal length of the division to the diameter will then express this normal length itself, and the proportion constituting the limit of stability will express the length corresponding to this limit. This admitted, let us resume the conclusion at which we arrived at the commencement of the preceding section, which conclusion we shall consequently express here by stating that in the case of all liquids the normal length of the divisions always exceeds the limit of stability; we must recollect, in the second place, that the sum of the lengths of one constriction and one dilatation is equal to that of a division, (§ 57;) and, thirdly, at the first moment of the transformation the length of one constriction is equal to that of a dilatation, (§ 46.) Now, it follows from all these propositions, that when the transformation of a cylinder begins to take place, the length of a single portion, whether constricted or dilated, is necessarily greater than half the limit of stability; consequently the sum of the lengths of three contiguous portions, for instance two dilatations and the intermediate constriction, is once and a half greater than this same limit. Hence, lastly, if the distance of the solid bases is comprised between once and once and a half the limit of stability, it is impossible for the limit of stability to give rise to three portions, and it will consequently only be able to produce a single dilatation in juxtaposition with a single constriction. This, in fact, as we have seen, always took place in regard to the cylinder, in § 46, which was evidently in the above condition, and the want of symmetry in its transformation now becomes explicable.

62. As stated at the conclusion of § 48, we have yet to describe a remarkable fact which always accompanies the end of the phenomenon of the transformation of a liquid cylinder into isolated masses.

In the transformation of large cylinders of oil, whether imperfect or exact, (§ 44 to 46,) when the constricted part is considerably narrowed, and the separation seems on the point of occurring, the two masses are seen to flow back rapidly towards the rings or the disks; but they leave between them a cylindri-

cal line which still establishes, for a very short time, the continuity of the one with the other, (Fig. 28;) this line then resolves itself into partial masses. It

Fig. 28.

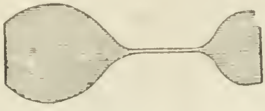


Fig. 29.

generally divides into three parts, the two extreme ones of which become lost in the two large masses, the intermediate one forming a spherule, some millimeters in diameter, which remains isolated in the middle of the interval which separates the large masses; moreover, in each of the intervals between this spherule and the two large masses, another very much smaller spherule is seen, which indicates that the separation of the parts of the above line is also effected by attenuated lines. Fig. 29 (Pl. VIII) represents this ultimate state of the liquid system. The same effects are produced when the resolution of the thin and elongated cylinder of oil of § 47 into spheres occurs, only there is in one or the other of the intervals between the spheres frequently a larger number of spherules, and, besides, the formation of the principal line is less easily observed, in consequence of the more rapid progress of the phenomena. Lastly, in the case of our cylinders of mercury, the resolution into spheres takes place also in too short a time to allow of our perceiving the formation of the lines; but we always find, in several of the intervals between the spheres, one or two very minute spherules, whence we may conclude that the separation is effected in the same manner.*

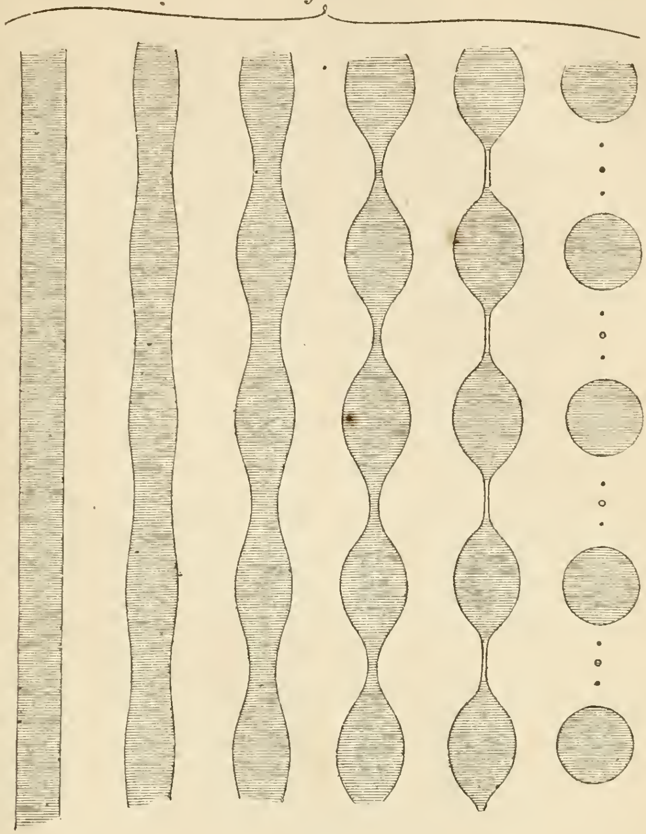
* We cannot avoid recognizing an analogy between the phenomenon of the formation of liquid lines and that of the formation of laminæ. In fact, in the experiment in § 23, for instance, the plane layer begins to be formed when the two opposite concave surfaces are almost in contact with each other at their summits; and in the resolution of a cylinder into spheres, the formation of the lines commences when all the meridional sections of the figures almost touch each other by the summits of their concave portions.

When treating of the layers, we have considered their formation as indicating a kind of tendency towards a particular state of equilibrium, which results from the circumstance that in the case of the thin part of the liquid system the ordinary law of pressure is modified. For the analogy between the two orders of phenomena to be complete, it would, therefore, be necessary that excessively delicate liquid lines should connect thick masses, and should thus form with these masses a system *in equilibrio*, notwithstanding the incompatibility of this equilibrium with the ordinary law of pressures. Now, we shall show that this equilibrium is in reality possible, at least theoretically. Let us always take as example the resolution of our unstable cylinder into partial masses. When the cylindrical lines form, their diameter is even then very small in comparison with the dimensions of the thick masses; consequently their curvature in the direction perpendicular to the axis is very great in comparison with the curvature of these masses. The pressure corresponding to the lines is then originally much greater than those corresponding to the thick masses, whence it follows that the liquid must be driven from the interior of the lines towards these same masses, and that the lines, like the layers, ought to continue diminishing. Moreover, their curvatures, and consequently their pressure augmenting in proportion as they become more attenuated, their tendency to diminish in thickness will go on increasing, and consequently if we disregard the instability of the cylindrical form, we see that they must become of an excessive tenuity. But I say that the augmentation of the pressure will have a limit, beyond which this pressure will progressively diminish, so that it may become equal to that which corresponds to the thick parts of the liquid system.

In fact, without having recourse to theoretical developments, it is readily seen that if the diameter of the line becomes less than that of the sphere of the sensible activity of the molecular attraction, the law of the pressure must become modified, and, the diameter continuing to decrease, the pressure must finish by also progressively diminishing, notwithstanding the increase of the curvatures, in consequence of the diminution in the number of attracting molecules. Hence the pressure may diminish indefinitely; for it is clear that it would entirely vanish if the diameter of the line became reduced to the thickness of a single molecule. Those geometricians who study the theory of capillary action know that the formulæ

As we are now acquainted with the entire course which the transformation of a liquid cylinder into isolated spheres must take, we can represent it graphi-

Fig. 30.



cally. Fig. 30 represents the successive forms through which the liquid figure passes, commencing with the cylinder up to the system of isolated spheres and

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of this theory cease to be applicable in the case of very great curvatures, or those the radii of which are comparable to that of molecular attraction. Now, it follows from what has been stated, that we may always suppose the thinness of the line to be such that the corresponding pressure may be equal to that existing in thick masses which have attained a state of equilibrium. In this case, admitting that the lines are mathematically regular, so that the pressure there may be everywhere rigorously the same, consequently that they have no tendency to resolve themselves into small partial masses, equilibrium will necessarily exist in the system. In this case the form of the thick masses will not be mathematically spherical; for their surface must become slightly raised at the junctures with the lines by presenting concave curvatures in the meridional direction. This form will be the same as that of an isolated mass, traversed diametrically by an excessively minute solid line, (§ 10.) This system, like those into the composition of which layers enter, is composed of surfaces of a different nature; but this heterogeneity of form becomes possible here, as in the case of the layers, in consequence of the change which the law of pressures undergoes in passing from one to another kind of surface.

We can, moreover, understand that the equilibrium in question, although possible theoretically, as we have shown, can never be realized, in consequence of the cylindrical form of the lines. The same does not apply to the case of the plane layers; for, as we shall show in the following series, the plane surfaces are always surfaces of stable equilibrium, whatever may be their extent.

of spherules. This figure refers to the case of a very slightly viscid liquid, such as water, alcohol, &c., and where the convex surface of the cylinder is perfectly free; consequently, in accordance with the probable conclusion with which § 60 terminates, the proportion of the length of the divisions to the diameter has been taken as equal to 4.

The phenomenon of the formation of lines and their resolution into spherules is not confined to the case of the rupture of the equilibrium of liquid cylinders; it is always manifested when one of our liquid masses, whatever may be its figure, is divided into partial masses. This is the manner in which, for instance, in § 29 of the preceding memoir the minute masses which were then compared to satellites are formed.* The phenomenon under consideration is also produced when liquids are submitted to the free action of gravity, although it is then less easily shown. For instance, if the rounded end of a glass rod be dipped in ether, and then withdrawn carefully in a perpendicular direction, at the instant at which the small quantity of liquid remaining adherent to the rod separates from the mass, an extremely minute spherule is seen to roll upon the surface of the latter. Lastly, the phenomenon in question is of the same nature as that which occurs when very viscid bodies are drawn into threads, as glass softened by heat, except that in this case the great viscidity of the substance, and moreover the action of cold, which solidifies the thread formed, maintains the cylindrical form of the latter and allows of its acquiring an indefinite length.

63. To complete the study of the transformation of liquid cylinders into isolated spheres, it still remains for us to discover the law according to which the duration of the phenomenon varies with the diameter of the cylinder, and to endeavor to obtain at least some indications relative to the absolute value of this duration in the case of a cylinder of a given diameter, composed of a given liquid, and placed in given circumstances.

We can understand, *à priori*, that when the liquid and the external circumstances are the same, and supposing the length of the cylinder to be always such that the divisions assume exactly their normal length, (§ 53,) the duration of the phenomenon must increase with the diameter; for the greater this is, the greater the mass of each of the divisions, and, on the other hand, the less the curvatures upon which the intensities of the configuring forces depend. It is true that the surface of each of the divisions increases also with the diameter of the cylinder; consequently it is the same with the number of the elementary configuring forces; but this augmentation takes place in a less proportion than that of the mass. This we shall proceed to show more distinctly. Under the above conditions two cylinders, the diameters of which are different, will become divided in the same manner; *i. e.*, the proportion of the length of a division to the diameter will be the same in both parts, (§ 55.) Now, it may be considered as evident that the similitude in figure will be maintained in all the phases of the transformation; this is, moreover, confirmed by experiment, as we shall soon see. Hence it follows at each homologous instant of the transformations of the two cylinders the respective surfaces of the divisions will always be to each other as the squares of the diameters of these cylinders, whilst the masses, which evidently remain invariable throughout the entire duration of the phenomena, will always be to each other as the cubes of these diameters. Thus, at each homologous instant of the respective transformations, the extent of the superficial layer of a division, consequently the number of the configuring forces which emanate from each of the elements of this layer, change from one figure to the other only in the proportion of the squares of the primitive diameters of

* It is clear that this mode of formation is entirely foreign to La Place's cosmogonic hypothesis; therefore we have had no idea of deducing from this little experiment, which only refers to the effects of molecular attraction, and not to those of gravitation, any argument in favor of the hypothesis in question—an hypothesis which, in other respects, we do not adopt.

these figures; whilst the mass of a division, all the parts of which mass receive, under the action of the forces in question, the movements constituting the transformation, changes in the proportion of the cubes of these diameters. As regards the intensities of the configuring forces, we must remember, first, that the measure of that which corresponds to one element of the superficial layer has (§ 59) for its expression $\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right)$. This granted, if, at an homologous instant in the transformations of the two figures, we take upon one of the divisions of each of the latter any point similarly placed, it is clear from the similitude of these figures that the principal radii of curvature corresponding to the point taken upon the second will be to those corresponding to the point taken upon the first in the proportion of the diameters of the original cylinders, so that if this proportion be n, and the radii relating to the point of the first figure be R and R′, those belonging to the point of the second will be nR and nR′; whence it follows that the measure of the two configuring forces corresponding to these points will be respectively $\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right)$, and $\frac{A}{2}\left(\frac{1}{nR}+\frac{1}{nR'}\right)=\frac{1}{n}\cdot\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right)$. Thus, in passing from the first to the second figure, the intensities of the elementary configuring forces in all the phases of the transformation will be to each other in the inverse proportion of the diameters of the cylinders.

I have convinced myself, by means of cylinders of mercury 1.05 millimeters and 2.1 millimeters in diameter, (§ 54 and 55,) that the duration of the phenomenon increases, in fact, with the diameter: although the transformation of these cylinders is effected very rapidly, yet we have no difficulty in recognizing that the duration relating to the greater diameter is greater than that which refers to the least.

64. As regards the law which governs this increase in the duration, it would undoubtedly be almost impossible to arrive at its experimental determination in a direct manner, *i. e.*, by measuring the times which the accomplishment of the phenomenon would require in the case of two cylinders of sufficient length to allow of their being respectively converted into several complete isolated spherules, and of their satisfying the conditions indicated at the commencement of the preceding section. In fact I can hardly see any method of realizing such cylinders without giving them very minute diameters, like those of our cylinders of mercury, and then their duration is too short to allow of our obtaining the proportion with sufficient exactness.

But we may be able to arrive at the same result, but with certain restrictions, which we shall mention presently, by means of two short cylinders of oil formed between two disks, (§ 46;) there is nothing to prevent these cylinders from being obtained of such diameters as to render the exact measure of the durations easy. In the transformation of a cylinder of this kind, only a single constriction and a single dilatation are produced; but as in the transformation of cylinders which are sufficiently long to furnish several complete isolated spheres, the phases through which the constrictions and the dilatations pass are the same for all, we need only consider one constriction and one dilatation. We can understand that the relative dimensions of the two solid systems ought to be such, that the relation between the distance of the disks and their diameters is the same in both parts, in order that similitude may exist between the two liquid figures at their origin and at each homologous instant of their transformations.

Before giving an account of the employment of these figures of oil for the determination of the law of the durations, we shall take this opportunity of making several important remarks. We shall only require to make use of the law in question in that case, which in other respects is the most simple, where

the cylinders are formed *in vacuo* or in air, and are free from all external resistance, or, in other words, free upon the whole of their convex surface. Now our short cylinders of oil are formed in the alcoholic liquid, and it might be asked whether this circumstance does not exert some influence upon the proportion of the durations corresponding to a given proportion between the diameters of these cylinders. At first, a greater or less portion of the alcoholic liquid must be displaced by the modifications of the figures, so that the total mass to be moved in a transformation is composed of the mass of oil and this portion of the alcoholic liquid; but it is clear that in virtue of the similitude of the two figures of oil and that of their movements, the quantities of surrounding liquid respectively displaced will be to each other exactly, or at least apparently, as the two masses of oil; so that the relation of the two entire masses will not be altered by this circumstance. Hence it is very probable that this circumstance will no longer exert any influence upon the proportion of the durations, except that the absolute values of these durations will be greater. On the other hand, the mutual attraction of the two liquids in contact diminishes the intensities of the pressures, (§ 8,) and consequently the configuring forces; but it is easy to see that this diminution does not alter the relation of these intensities in the two figures. For let us imagine that at an homologous instant of the two transformations the alcoholic liquid becomes suddenly replaced by the oil, and let us conceive in the latter the surfaces of the two figures as they were at that instant. The configuring forces will then be completely destroyed by the attraction of the oil outside these surfaces, or, in other words, the external attraction will be at each point equal and opposite to the internal configuring force. If we now replace the alcoholic liquid, the intensities of the external attractions will change, but they will evidently retain the same relations to each other; whence it follows that those corresponding to two homologous points taken upon both the figures will still be to each other as the configuring forces commencing at these points; so that in fact the respective resultants of the external and internal actions at these two same points will be to each other in the same proportion as the two internal forces alone. Thus the attractions exerted upon the oil by the surrounding alcoholic liquid will certainly diminish the absolute intensities of the configuring forces, but they will not change the relations of these intensities, consequently they may be considered as not exerting any influence upon the durations. But it is clear that this cause will nevertheless greatly increase the absolute values of the latter. For the two reasons which we have explained, the presence of the alcoholic liquid will then increase the absolute values of the two durations to a considerable extent; but we may admit that it will not alter the relation of these values, so that this proportion will be the same whether the phenomenon take place *in vacuo* or in air. We shall, therefore, consider the law which we deduce from our experiments upon short cylinders of oil as independent of the presence of the surrounding alcoholic liquid, and this will be found to be supported by the nature of the law itself.

But the exact formation of our short cylinders of oil requires (§ 46) that in these cylinders the proportion between the length and the diameter, or what comes to the same thing, between the sum of the lengths of the constriction and the dilatation and the diameter, exceeds but little the limit of stability. Now, in the transformation of cylinders sufficiently long to furnish several spheres, which would be formed *in vacuo* or in the air, and free upon their entire convex surface, and the divisions of which have their normal length, the proportion of the sums of the lengths of one constriction and one dilatation to the diameter, which proportion is the same as that of the length of one division to the diameter, would vary with the nature of the liquid, (§ 59,) and we are ignorant whether the law of the durations is independent of the value of this proportion. The law which we shall obtain in regard to short cylinders of oil can only there-

fore be legitimately applied to cylinders of sufficient length to furnish several spheres supposed to be in the above conditions, in the case where these latter cylinders are formed of such a liquid that they would give for the proportion in question a value but little greater than that of the limit of stability.

Now this is the case of mercury, (§ 60,) and it is also very probable that of all other very slightly viscid liquids, (§ 60.) Thus the law given by the short cylinders of oil will be exactly or apparently that which would apply to cylinders of mercury of sufficient length to furnish several spheres, supposing the latter to be produced *in vacuo* or in air, free at the whole of their convex surface, and of such length that the divisions in each of them would assume their normal length. Moreover, the same law would be undoubtedly applicable to cylinders formed of any other very slightly viscid liquid, and supposed to be in the same conditions as the preceding.

The law may possibly be completely general, *i. e.*, it may apply to cylinders formed, always under the same circumstances, of any liquid whatever; but our experiments do not furnish us with the elements necessary to decide this question. Lastly, the transformation of our short cylinders presents a peculiarity which entails another restriction. The two final masses into which a cylinder of this kind resolves itself being unequal, the smallest acquires its form of equilibrium considerably before the other, so that the duration of the phenomenon is not the same. Hence we can only determine its duration up to the moment of the rupture of the line; consequently the proportion which we thus obtain for both cylinders will only be that of the durations of two homologous portions of the entire transformations. Moreover, the proportion of these partial durations is exactly that of which we shall have hereafter to make use.

65. I made the experiments in question by employing two systems of disks, the respective dimensions of which were to each other as one to two; in the former, the diameter of the disks was 15 millimeters, and they were 54 millimeters apart; and in the second their diameter was 30 millimeters, and their distance apart 108 millimeters. The cylinders formed respectively in these two systems were therefore alike, and, as I have previously stated, (§ 63,) these two figures exactly maintained their similarity, as far as the eye was capable of judging, in all the phases of their transformations. It sometimes happened that the cylinder, when apparently well formed, was not at all persistent and immediately began to alter; this circumstance being attributable to some slight remaining irregularity in the figure, I immediately re-established the cylindrical form,* and the time was only taken into account when the figure appeared to maintain this form for a few moments. Another anomaly then sometimes presented itself, which consisted in the simultaneous formation of two constrictions with an intermediate dilatation; this modification ceased when it had attained a very slightly marked degree, and the figure appeared to remain in the same state for a considerable period; † then one of the constrictions became gradually more marked, whilst the other disappeared, and the transformation afterwards went on in the usual manner. As this peculiarity constituted an exception to the regular course of the phenomenon, I ceased to reckon as soon as it showed itself, and I again re-established the cylindrical form. The estimation of the time was only definitively continued in those cases in which, after some persistence in the cylindrical form, a single constriction only was produced.

I repeated the experiment upon each of the two cylinders twenty times, in order to obtain a mean result. As soon as one transformation was completed,

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* See the second note to paragraph 46.

† We shall see, in the following series, to what this singular modification in the figure is owing.

I reunited the two masses to which it had given rise, and again formed the cylinder,* in order to proceed to a new measure of the time.

The number of seconds are given below; each expresses the time which elapsed from the moment of the transformation of the cylinder to that of the rupture of the line. These periods were determined by means of a watch, which beat the $\frac{1}{5}$ths of a second.

Cylinder 15 millims. in diameter.		Cylinder 30 millims. in diameter.	
″	″	″	″
25.0	36.4	59.6	51.6
26.6	32.0	73.0	68.0
28.0	30.4	57.0	73.6
30.0	24.6	61.0	61.8
24.8	32.6	67.8	53.0
35.2	33.8	60.0	58.0
27.0	33.8	63.6	63.8
30.0	20.2	54.2	60.0
30.4	28.6	61.0	52.6
29.8	32.6	52.6	55.2
Mean 29″.59.		Mean 60″.38.	

It is evident that the numbers relating to the same diameter do not differ sufficiently from each other to prevent our regarding the proportion of the two means as closely approximating to the true proportion of the durations. Now the proportion of these two means is 2.04, *i. e.*, almost exactly equal to that of the two diameters. Moreover, it is evident that in the case of each of the latter the greatest of the numbers obtained must correspond to that case where the cylinder is formed in the most perfect manner; consequently it is probable that the proportion of these two greatest numbers also closely approximates to the true proportion of the durations. Now, these two numbers are, on the one hand 36.4, and on the other 73.6, and their proportion is 2.02, which number differs still less from 2, or from the proportion of the diameters.

We may, therefore, admit that the durations relating to these two cylinders are to each other as their diameters; whence we deduce this law, that the partial duration of the transformation of a cylinder of the same kind is in proportion to its diameter.

I have said (§ 64) that the law thus obtained would of itself furnish a new motive for believing that it would not change if our short cylinders of oil were produced *in vacuo* or in air. In fact the proportionality to the diameter is the simplest possible law; and, on the other hand, the circumstances under which the phenomenon is produced are less simple in the case of the presence of the alcoholic liquid than they would be in that of its absence; consequently, if the law changed from the first to the second, it would follow that a simplification in the circumstances would, on the contrary, induce a complication of the law, which is not very probable.

* This was effected by conducting the large mass towards the small one, by means of the ring of which I spoke in the first note to paragraph 46. But care must be taken to prevent the ring, on separating from the liquid figure, from carrying away with it any perceptible quantity of oil; for this purpose, instead of making the entire ring adhere to the great mass, I left a small portion of the latter free, and, as its action was then insufficient to make the large mass reach the other, I aided it by gently pushing the oil with the extremity of the point of the syringe. On withdrawing the ring after the reunion of the two masses, only a very small spherule of oil separated from it in the alcoholic liquid, which in the next experiment I again united to the rest of the oil by means of the ring itself, as also the largest of the spherules arising from the transformation of the line.

We may, therefore, I think, legitimately generalize the above law in accordance with the whole of the remarks made in the preceding section, and deduce the following conclusions:

1. If we conceive a cylinder of mercury formed *in vacuo* or in air, of sufficient length to furnish several spheres, its convex surface being entirely free, and its length such that the divisions assume exactly their normal length, the time which will elapse from the origin of the transformation to the instant of the rupture of the lines will be exactly or apparently proportional to the diameter of this cylinder.

2. The same very probably applies to a cylinder formed of any other very slightly viscid liquid, as water, alcohol, &c., and supposed to exist under the same circumstances.

3. It is possible that this law is completely general, *i. e.*, applicable to a cylinder formed, always under the same circumstances, of any kind of liquid whatever; but our experiments leave us in doubt on this point.

66. Let us now enter upon the consideration of the absolute value of the time in question for a given diameter, the cylinder always being supposed to be produced *in vacuo* or in air, of sufficient length to furnish several spheres, its entire convex surface free, and its length such that its divisions assume their normal length. It is clear that this absolute value must vary according to the nature of the liquid; for it evidently depends upon the density of the latter, upon the intensity of its configuring forces, and, lastly, upon its viscidity. The experiments which we have detailed give with regard to oil a very remote superior limit; this results, first, from the two causes which we have mentioned in § 64, and which are due to the presence of the alcoholic liquid; but with these two causes is connected a third, which we must make known. If we imagine a cylinder of oil formed under the above conditions, the sum of the lengths of a constriction and a dilatation will necessarily be much greater in regard to this cylinder than in regard to one of our short cylinders of oil of the same diameter; for in the former this sum is equivalent to the length of a division; and in consequence of the great viscidity of the oil, this latter quantity must greatly exceed the length corresponding to the limit of stability. Now, it may be laid down as a principle, that, all other things being equal, an increase in the sum of the lengths of a constriction and a dilatation tends to render the transformation more rapid, and consequently to abbreviate the total and partial durations of the phenomenon. In fact, for a given diameter, the more the sum in question differs from the length corresponding to the limit of stability, the more the forces which produce the transformation must act with energy; moreover, as the transformation ceases to take place immediately above the limit of stability, the duration of the phenomenon may then be considered as infinite, whence it follows that when this limit is exceeded, the duration passes from an infinite to a finite value, consequently it must decrease rapidly as it deviates from this limit; lastly, this is also confirmed by the results of observation, as we shall show hereafter. Thus, even if it were possible to form *in vacuo* or in air one of our very short cylinders of oil, consequently to eliminate the two causes of retardation due to the presence of the alcoholic liquid, the duration relative to the cylinder would still exceed that which would relate to a cylinder of oil of the same diameter formed under the conditions we have supposed.

I have said that the principle above established is confirmed by experiment, *i. e.*, for the same diameter, the same liquid, and the same external actions, if any exist; when, from any cause, the sum of the lengths of a constriction and a dilatation augments, the total and partial durations of the transformation become less. We shall proceed to make this evident. In the experiments of the preceding section, the partial duration relating to the cylinder, the diameter of which was 15 millimeters, was, for instance, about 30 seconds, the mean, as shown by the table. Consequently, if we were to form in the alcoholic liquid a similar

cylinder of oil, the diameter of which is 4 millimeters, the partial duration of this, in virtue of the law which we have found, would be nearly equal to $\frac{30'' + 4}{15} = 8''$. Now, in the nearly cylindrical figure of oil of § 47, which figure is also formed in the alcoholic liquid, the mean diameter was (§ 56) about 4 millimeters. In this and the preceding figure, the diameter, the liquid, and the external actions then are the same; but in the former, the sum of the lengths of the constriction and the dilatation would only be equal to 4 millimeters, + 3.6 = 14.4 millimeters, whilst in the second, this sum, which is equivalent to the length of a division, was (§ 56) approximatively 66.7 millimeters. Now, on observing this latter figure, we recognize easily that the duration of its transformation is much less than 8″. In truth, from the nature of the experiment, it is impossible with regard to this same figure to fix upon the commencement of the formation of a given constriction or dilatation, so that the complete duration should considerably exceed that which would be deduced by the simple inspection of the phenomenon; but the latter does not amount to one second, and there cannot be any doubt that it would be going too far to extend the complete duration, and *à fortiori*, the portion which terminates at the rupture of the lines, to two seconds. Thus in the case we have just considered, the sum of the length of a constriction and a dilatation becoming about four and a half times greater, the partial duration becomes at least four times less.

67. But if, in reckoning the absolute duration in the case of one of our short cylinders of oil, we only obtain with regard to this liquid one upper limit, and this much too high, the cylinder of mercury in § 55 (which cylinder is formed in the air, and the length of which in proportion to the diameter is sufficient for the divisions to have assumed exactly, or very nearly, their normal length) will furnish us, on the contrary, in regard to this latter liquid, with a limit which is probably more approximative and which will be very useful to us.

First, in the case of this cylinder, the diameter of which, as we have said, was 2.1 millimeters, the transformation does not take place in a sufficiently short time for us to estimate with any exactitude the total duration of the phenomenon; I say the total duration, because in so rapid a transformation it would be very difficult to determine the instant at which the rupture of the lines occurs. To approximate as closely as possible to the value of this total duration, I have had recourse to the following process.

By successive trials, I regulated the beats of a metronome in such a manner, that on rapidly raising, at the exact instant at which a beat occurs, the system of glass strips belonging to the apparatus serving to form the cylinder, (§ 50 and 51,) the succeeding beat appeared to me to coincide with the termination of the transformation; then having satisfied myself several times that this coincidence appeared very exact, I determined the duration of the interval between two beats, by counting the oscillations made by the instrument during two minutes, and dividing this time by the number of oscillations. I thus found the value 0″.39 for the interval in question. The total duration of the transformation of our cylinder of mercury may therefore be valued approximatively at 0″.39, or more simply, at 0″.4.

But the entire convex surface of this cylinder is not free, and its contact with the plate of glass must exert an influence upon its duration, both directly as well as by the increase which it produces in the length of the divisions. Let us examine the influence in question under this double point of view.

The direct action of the contact with the plate is undoubtedly very slight; for as soon as the transformation commences, the liquid must detach itself from the glass at all the intervals between the dilated parts, so as only to touch the solid plane by a series of very minute surfaces belonging to these dilated parts; consequently, if the direct action of the contact of the plate were alone eliminated, *i. e.*, if we could manage so that the entire convex surface of the cylinder should

be free, but that the divisions formed in it should acquire the same length as before, the total duration would scarcely be at all diminished.

There still remains the effect of the elongation of the divisions. The length of the divisions of our cylinder is equal to 6.35 times the diameter, (§ 56,) whilst, according to the hypothesis of the complete freedom of the convex surface, this length would very probably be less than four times the diameter, (§ 60.) Now, in virtue of the principle established in the preceding section, this increase in the length of the divisions necessarily entails a diminution in the duration, which diminution is more considerable in proportion as it occurs in the vicinity of the limit of stability; consequently, if it could be managed so that the elongation in question should not exist, the total duration would be very considerably increased. Thus the suppression of the direct action of the contact of the plate would only produce a very slight diminution of the total duration; and the annihilation of the elongation of the divisions would produce, on the other hand, a very considerable increase in this same duration. If, then, these two influences were simultaneously eliminated, or, in other words, if the entire convex surface of our cylinder were free, the total duration of our transformation would be very considerably greater than the direct result of observation.

Now, the quantity which we have to consider is the partial, and not the total duration; but, under the same circumstances, the first must be but little less than the second; for when the lines are about to break, the masses between which they extend even then approximate to the spherical form; consequently, in accordance with the conclusion obtained above, we must admit that the partial duration under our present consideration, *i. e.*, that referring to the case of the complete freedom of the convex surface of the cylinder, would still exceed considerably the total duration observed, *i. e.*, $0''.4$.

In starting from this value $0''.4$ as constituting the lower limit corresponding to a diameter of 2.1 millimeters, the law of the proportionality of the partial duration to the diameter will immediately give the lower limit corresponding to any other diameter; we shall find, *e. g.*, that for 6 millimeters this limit would be $\frac{0''.4 + 10}{2.1} = 1''.9$, or more simply $2''$.

If, then, we imagine a cylinder of mercury a centimeter in diameter, formed *in vacuo* or in air, of sufficient length to furnish several spheres, entirely free at its convex surface, and of such a length that its divisions assume their normal length, the time which will elapse from the origin of the transformation of this cylinder to the instant of the rupture of the lines will considerably exceed two seconds.

68. It will not be superfluous to present here a *resumé* of the facts and laws which the experiments we have described have led us to establish with respect to unstable liquid cylinders.

1. When a liquid cylinder is formed between two solid bases, if the proportion of its length to its diameter exceeds a certain limit, the exact value of which is comprised between 3 and 3.6, the cylinder constitutes an unstable figure of equilibrium.

The exact value in question is that which we denominate *the limit of stability of the cylinders.*

2. If the length of the cylinder is considerable in proportion to its diameter, it becomes spontaneously converted, by the rupture of equilibrium, into a series of isolated spheres, of equal diameter, equally distant, having their centres upon the right line forming the axis of the cylinder, and in the intervals of which, in the direction of this axis, spherules of different diameters are placed; except that each of the solid bases retains a portion of a sphere adherent to its surface.

3. The course of the phenomenon is as follows: The cylinder at first gradually swells at those portions of its length which are situated at equal distances from

each other, whilst it becomes thinner at the intermediate portions, and the length of the dilatations thus formed is equal, or nearly so, to that of the constrictions; these modifications become gradually more marked, ensuing with accelerated rapidity, until the middle of the constrictions has become very thin; then, commencing at the middle, the liquid rapidly retires in both directions, still, however, leaving the masses united two and two by an apparently cylindrical line; the latter then experiences the same modifications as the cylinder, except that there are in general only two constrictions formed, which consequently include a dilatation between them; each of these little constrictions becomes in its turn converted into a thinner line, which breaks at two points and gives rise to a very minute isolated spherule, whilst the above dilatation becomes transformed into a larger spherule; lastly, after the rupture of the latter lines, the large masses assume completely the spherical form. All these phenomena occur symmetrically as regards the axis, so that, throughout their duration, the figure is always a figure of revolution.

4. We denominate *divisions* of a liquid cylinder, those portions of the cylinder, each of which must furnish a sphere, whether we conceive these portions to exist in the cylinder itself, before they have begun to be apparent, or whether we take them during the transformation, *i. e.*, whilst each of them is becoming modified so as to arrive at the spherical form. The length of a division consequently measures the constant distance which, during the transformation, is included between the necks of two adjacent constrictions.

Moreover, by *normal length of the divisions*, we denominate that which the divisions would assume, if the length of the cylinder to which they belong were infinite.

In the case of a cylinder which is limited by solid bases, the divisions also assume the normal length when the length of the cylinder is equal to the product of this normal length by a whole number, or rather a whole number and a half. Then, if the second factor is a whole number, the transformation becomes disposed in such a manner that during its accomplishment the figure terminates on one side with a constriction, and on the other with a dilatation; if the second factor is composed of a whole number and a half, the figure terminates on each side in a dilatation. When the length of the cylinder fulfils neither of these conditions, the divisions assume that length which approximates the most closely possible to the normal length, and the transformation adopts that of the two above dispositions which is most suitable for the attainment of this end.

5. In the case of a cylinder of a given diameter, the normal length of the divisions varies with the nature of the liquid, and with certain external circumstances, such as the presence of a surrounding liquid, or the contact of the convex surface of the cylinder with a solid plane. In all the subsequent statements we shall take the simplest case, *i. e.*, that of the absence of external circumstances; in other words, we shall always suppose that the cylinders are produced *in vacuo* or in air, and that they are free as regards their entire convex surface.

6. Two cylinders of different diameters, but formed in the same liquid, and the lengths of which are such that the divisions assume in each of them their normal length, become subdivided in the same manner, *i. e.*, the respective normal lengths of the divisions are to each other as the diameters of these cylinders. In other words, when the nature of the liquid does not change, the normal length of the divisions of a cylinder is proportional to the diameter of the latter.

The same consequently applies to the diameter of the isolated spheres into which the normal divisions become converted, and to the length of the intervals which separate these spheres.

7. The proportion of the normal length of the divisions to the diameter of the cylinder always exceeds the limit of stability.

8. This proportion is greater as the liquid is more viscid and as the configuring forces in it are weaker.

9. In the case of a cylinder of mercury, this proportion is much less than 6, and we may admit that it is less than 4.

In the case of a cylinder composed of any other very slightly viscid liquid, such as water, alcohol, &c., it is very probable that the proportion in question is very nearly 4. Hence, in the case of the latter liquids, we have for the probable approximative value of the proportion of the diameter of the isolated spheres resulting from the transformation and the diameter of the cylinder, the number 1.82; and for that of the proportion of the distance of two adjacent spheres to this same diameter, the number 2.18.

10. If mercury is the liquid, and the divisions have their normal length, the time which elapses between the origin of the transformation and the instant of the rupture of the lines, is exactly or apparently proportional to the diameter of the cylinder.

This law very probably applies also to each of the other very slightly viscid liquids.

This same law may possibly be general, *i. e.*, it may be applicable to all liquids; but our experiments leave this point uncertain.

11. For the same diameter, and when the divisions are always of their normal length, the absolute value of the time in question varies with the nature of the liquid.

12. In the case of mercury, and with a diameter of a centimeter, this absolute value is considerably more than two seconds.

13. When a cylinder is formed between two solid bases sufficiently approximated for the proportion of the normal length of the cylinder to the diameter to be comprised between once and once and a half the limit of stability, the transformation gives only a single constriction and a single dilatation; we then obtain for the final result only two portions of a sphere which are unequal in volume and curvature, respectively adherent to solid bases, besides interposed spherules.

(TO BE CONTINUED IN THE NEXT REPORT.)

HISTORY OF DISCOVERY RELATIVE TO MAGNETISM.

COMPILED FOR THE INSTITUTION PRINCIPALLY FROM THE "AUS DER NATUR."

There are two great forces of nature everywhere present and at every moment exerting their influence, namely, gravitation and magnetism. They are similar in many particulars, all pervading and perhaps equally powerful. The magnetic phenomena of the earth, however, do not manifest themselves as freely to the senses as those of gravitation, and the naturalist is obliged to employ refined, and, in some cases, complicated apparatus to study the laws of its operation. In this article we purpose to present to our readers a sketch of the earlier discoveries relative to magnetism, and in doing so we shall also briefly explain the general principles of the science.

There is found in different parts of the earth a mineral of a dark color, principally composed of iron and oxygen, which has long been an object of interest to the ignorant as well as the learned, principally on account of the attraction which it exhibits for iron, and the wonderful property which it imparts to steel needles of pointing toward the poles of the earth. Its composition may be expressed chemically by the formulæ $Fe\,O + Fe_2\,O_3$, being a compound of the first and second oxide of iron. It is called loadstone, and occurs most generally in primary mountains of gneiss; chlorite slate, in primitive limestone, and sometimes in considerable masses in serpentine, and in trap. It is found in great quantity and purity at Rosslay, in Sweden, in Corsica, on the island of Elba, in Norway, Siberia, Saxony, Bohemia, and in the Hartz mountains. A hill in Swedish Lapland, and Mount Pumachanche, in Chili, are said to consist almost entirely of magnetic ore. Extensive beds of magnetic iron ore are found in various places in the United States, and in some of these occur masses of the mineral possessing polarity; such as those at Marshall's island, Maine, at Magnet's Cove, Arkansas, at Goshen, Chester county, Pennsylvania, and Franklin, New Jersey.

It has been asserted that this mineral is not magnetic in its natural condition in the mine, but that the pieces only exhibit this property after having been exposed to the light; but this statement has not been verified, and is apparently at variance with well-established facts.

The specimens of this mineral are usually so hard that they produce fire when struck with steel, and it is this circumstance which renders them so difficult to be worked into proper form for exhibiting in the best manner the magnetic property.

The name magnet, by which the mineral is known to us, is said to be derived from Magnesia, a city in Asia Minor, where it was first found. The Roman poet Lucretius bears testimony to this in a passage of his celebrated poem on the nature of things, in which he states that the Greeks called this stone magnet because it was found in the country of the Magnesians.

This statement is much more probable than the account given by Pliny, who derives the name of magnet from Magnes, a herdsman, who, in guarding his flock on Mount Ida, found himself suddenly held fast to a magnetic rock by the iron nails in his shoes and the iron point of his staff. But whatever may be the origin of the names by which the magnet has been designated in different languages, it is a remarkable fact that they show distinctly the idea that pre-

vailed in every part of the world respecting the phenomenon of its attraction of iron. In those cases where the roots of the languages have no analogy whatever, the ideas expressed by the terms are often identical. In some, indeed, the attraction itself is alone expressed; but in the majority of cases the *motive* of attraction is embodied with it—a supposed affection for the iron—a love for it is expressed. It is the same in the European and Asiatic languages; and as the magnet is found in all, or nearly all, the countries of the Old World, we can only suppose that it arose in most cases in every language independently of any other. Nor is this peculiarity wholly confined to the names amongst nations of the most poetic temperament, since even the Chinese have the same idea in their *Thsu-chy* (the common name) or *lovestone*. What may appear most surprising is, that the name of the magnet seldom occurs in the older poetry of any country; but probably this arose from the unpoetic subject, namely, that of iron, with which it was coupled. In the poetry of later times, however, allusion to the magnet often occurs, and in several beautiful passages of our own it would be easy to point it out, both in expressing love and constancy—the former by its attractive, and the latter by its direct power. No phrase, indeed, is more familiar than to call the object of affection "the magnet."

From all the records which refer to the subject, we must conclude that the ancients had at an early period a knowledge of some of the more obvious phenomena of magnetism, and that they possessed magnets of considerable lifting power. They appear also to have been acquainted with the means of increasing the attractive power of the loadstone by the application to its poles of what is called an armature, that is, by applying pieces of soft iron to the parts of the stone which exhibited the greatest attraction, and which, as we shall hereafter see, are called its poles. Thus, Claudenus, in his work entitled Magnes, states that the wonderful stone gains power by contact with iron, and loses it again by the separation of this metal.

The same author describes a performance in a temple in which a statue of Venus, cut from a magnet, lifted an iron statue of Mars into the air. Lucian, in his work on the Syrian goddess, mentions a similar performance, in which a statue of Apollo was lifted before his eyes by the priests without being touched, and remained suspended in the air. Pliny also relates that Dinocrates, an architect of Ptolemy Philadelphus, commenced to build a temple at Alexandria, in honor of Arsinoe, sister of the King, of which the vault was to be built of magnets, so that an iron statue of the former might be suspended in the air. This temple, however, was not finished because both Ptolemy and his architect died before it could be completed.

According to Cedrenus and Augustine, a similar performance was actually exhibited in a temple of antiquity. The former asserts that the statue of an ancient god was held suspended by magnetic power in the serapium at Alexandria, and the latter, without mentioning any particular temple, states that the suspension was such as to cause the people to believe that the statue was soaring in the air. Matheolus, a commentator of Galenus, relates a similar story of the coffin of Mahomet, which is said to soar in the air in a sanctuary built of magnetic stones.

These statements, though probably founded on a limited knowledge of magnetic phenomena, are now known to be fabulous, since, after a full investigation of the subject, we are certain that it is impossible to suspend in mid air, without contact, a piece of iron by means of magnetism. The magnetic power diminishes very rapidly with the distance from the poles, and, in order that the iron should be suspended, it must be placed at the exact point in space at which the attraction of the magnet upwards would be equal to the force of gravity downwards; but if it could be placed in this position, it would not retain it for a moment, since the slightest jar or the least breath of air would disturb the equilibrium, and the iron would immediately fall to the floor, or spring up into

contact with the poles of the magnet. Some plausibility was, however, given to these stories, because magnets have been obtained which could sustain a heavy weight of iron when the latter was in contact with the poles of the latter. Thus, Wolf mentions examples of natural magnets which could support, by means of an armature, from sixteen to forty times, and even three hundred and twenty times their own weight. Dufay had in his possession a magnet of nine pounds in weight, which could hold seventy-six pounds. As a general rule, smaller magnets can support comparatively more than larger ones. Such, for example, as weigh from twenty to thirty grains will sometimes support fifty times their weight, whilst magnets weighing two pounds scarcely ever sustain ten times their own weight. According to Dr. Martin, Sir Isaac Newton had a magnet which was set in a finger-ring, and which, though only of three grains in weight, could hold seven hundred and forty-six grains. In the philosophical cabinet of the university at Dorpat there is a magnet weighing forty pounds, including the armature and a copper case, which is able to sustain eighty-seven pounds. A still larger one is found in Tyler's museum, which weighs three hundred and seven pounds, the armature inclusive, and holds more than two hundred and thirty pounds. Not less considerable was the magnet which John I, King of Portugal, received as a present from the Emperor of China, which weighed a little over thirty-eight pounds, and was able to support two hundred and two pounds.

But to return to the direct continuation of our history, we should state that a tradition of a very ancient date still exists in China respecting a mountain of magnetic ore rising in the midst of the sea, the intensity of attraction of which is so great as to draw the nails and iron bolts with which the planks of a ship are fastened together from their places with such force as to cause the vessel to fall to pieces. This tradition is not confined to China, but is very general throughout all Asia; and the Chinese historians assign to the mountain a specific place which they call *Tchang-haï*, the southern sea, between *Tonquin* and *Cochin-China*. Ptolemy, also, in a remarkable passage in his geography, places this mountain in the Chinese seas. In a work attributed to St. Ambrose there is an account of one of the islands of the Persian Gulf, called Mammoles, in which the magnet is found, and the precautions necessary to be taken in building ships without iron to navigate in that vicinity is distinctly specified. In two passages of the work of the Arabian geographer, *Cherif-Edrïsï*, and in a remarkable one in the apocryphal Arabian translation of the "Treatise on Stones," attributed to Aristotle, the existence of this mountain is again specifically stated. A reference to it also occurs in Vincent de Beauvais, a French writer, who had been in the holy wars; and, after his time, in the works of a great number of European writers.

A circumstance remarkable enough is, that the Chinese writers place this magnetic mountain in precisely the same geographical region in which it is stated to exist by the author of the voyages of *Sinbad the Sailor*. This has been justly looked upon as a confirmation of an opinion as to the oriental origin of a great number of the tales, half fiction, half fact, which are so universally diffused among the legendary literature of every country as to appear indigenous in each of them. We would not, however, go to the extent of saying that *all* our nursery fictions are derived from the east, though it cannot be denied that a great number of them are of oriental origin.

It is not surprising that the magnet which exhibited such extraordinary physical effects should have attributed to it wonderful moral and medicinal powers. Accordingly we find the belief entertained that it could enable its possessor to gain the confidence of princes, the affection of women, and to secure conjugal love, as well as cure the gout, the headache, and the heartache. In a little book of secrets, extracted from Albertus Magnus and others, was one to ascertain whether your sweetheart did really love you, and another to dis-

cover whether your bride had married you from motives of affection or otherwise. Both were to be effected by the mystical use of the magnet.

It has already been mentioned that on the surface of a magnet there are two points at which the attractive power manifests itself with the greatest intensity, and that these points are called poles. If a piece of soft iron is presented to one of these poles, the iron itself will become a magnet of inferior power, will exhibit two poles, and attract a second piece of iron; this second piece of iron will in turn become a magnet, and attract a third, and so on. The power may thus be developed in a series of iron bars placed end to end, provided the original magnet has considerable power. If, instead of bars of iron, small particles, such as filings of iron, be placed under the influence of the magnet, they will adhere together in masses, and form a kind of beard around the poles, or, if they are sprinkled on a sheet of paper placed over the magnet, they will be attracted to the poles and to each other, forming curves of great regularity and beauty.*

These experiments were known to the ancients, and Lucretius must have seen them performed by the priests, since he describes them minutely in his poem to which we have previously alluded. In this he states that iron filings contained in a brass basin appeared to boil when a magnet was moved under them; that a row of iron rings would hang one below the other on a magnet, and that these experiments were performed by the priests in connexion with the Samothracean mysteries. A similar experiment was exhibited at a festival held every ninth year in honor of Apollo at Thebes, in Boetia, which consisted in hanging one iron ball on another. These experiments were undoubtedly made by means of a strong magnet inducing its power in pieces of soft iron, the latter exhibiting the attraction as long as they were in metallic contact with the former, but immediately losing the power when the contact was severed.

It is only when the iron has been rendered hard by hammering or twisting that it is able to retain a small amount of magnetism. But if, instead of soft iron, bars of tempered steel are placed in contact with the pole of a magnet, they will at first not be attracted as powerfully as those of iron; but if they are allowed to remain in contact for some time, or if rubbed with the magnet, they will fully acquire the magnetic property, and retain it after they have been separated from the inducing magnet.

If a bar of steel, which has thus been rendered permanently magnetic, and of which its poles are at its ends, be placed on a piece of cork, and allowed to float horizontally on water, or if it be supported on a firm point at its centre of gravity, or, still more simply, by a fine thread, so as to have free motion in every direction horizontally, it will not remain at rest indifferently in any direction, but will turn itself so as to point with its poles to a definite region of the earth, the one to the north, and the other to the south. If two such movable magnetic bars are brought near each other, the poles of both which point to the north, and also those which point to the south, will repel each other, whilst the pole which points to the north in the one will attract the pole which points to the south in the other, and *vice versa*.

The directive property of a freely suspended magnetic bar towards certain points of the horizon, which is generally called the polarity of the needle, was not known to western nations as early as the attractive power of the magnet

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* A very interesting experiment, which may be called the exhibition of magnetic spectres, consists in tracing on a polished plate of steel, such as the blade of a wide handsaw, an image in outline with a pencil, and afterwards passing slowly and with some pressure along the lines of this image one of the tapered poles of a straight magnet of considerable power. If a sheet of white paper is afterwards pasted smoothly over this steel surface, and against this, while it is held vertically, fine iron filings are projected from a box with a perforated cover, the image will start into existence on the blank paper, as if by magic, in lines of bristling filings.

The image is interestingly shown by drawing a serpentine line on a long saw blade, to represent a snake; the configuration of the filings gives a peculiar effect to this exhibition.

for iron. It is true that King Solomon is said to have been acquainted with the use of the mariner's compass, and the Hebrew Parvaim, to which he sent his vessels for treasures, is said to have been no other country than Peru itself; but, since Solomon employed Phenician seamen, the compass would necessarily have been known to the Phenicians, and from these the Greeks and the Romans would most certainly have learnt its application.

The claims of the Chinese to the discovery of the directive power of the magnet, and its application to navigation, has long been affirmed and denied; but it has of late been defended by an author of much learning and ability, namely, Klaproth, in a letter to Humboldt. It is difficult to mention any useful contrivance which is not in some degree known to this singular people, or any period in history when they did not know it. The great obstacle which has stood in the way of admitting the claims of the Chinese to many of these inventions is the high antiquity to which their records profess to ascend, and their consequent incompatibility with our own received chronology; but whoever has looked with any degree of attention upon the fragments of their scientific history, and the incidental mention made of things which were familiar to the writers, but which did not form the principal object of the record, cannot fail to be struck with the apparent general consistency which runs through all their claims to high antiquity, and to be forced to the conclusion that there is still wanting a key to that consistency which is not furnished by the sweeping charge of the forgery of their annals.

It has been said that the fine arts of China appear more like being in a condition of gradual decay than in a state of freshness and energy, and that it may be possible that their arts, as well as those of Egypt, were transferred from some older people, who were in a condition of decline; but this is mere conjecture, unsupported by any evidence, either written or oral. In regard to the Chinese, it would appear, from the little progress they have made since they became known to history, and their want of knowledge and appreciation of the scientific principles on which art is founded, that their condition is just such as would be produced in an ingenious people in a long time by the accidental discoveries of facts, and their empirical application to the wants and conveniences of life. After a certain time, such a people would make no further progress; the facts which could be gathered from casual observation would be exhausted, and the advance in civilization, as well as the increase in population, would become exceedingly tardy.

Duhalde, in his account of China, states that the inhabitants of that country were acquainted with the polarity of the needle in the earliest times; that hundreds of years before our era they used, in their land excursions, an instrument in which the movable arm of a human figure invariably pointed towards the south, as a means of assistance in finding their way through the grass-covered plains of Tartary. Even as early as the third century of our era, about seven hundred years before the introduction of the mariner's compass into the European seas, it is asserted that Chinese vessels sailed on the Indian ocean, directed by magnetic polarity pointing towards the south. Humboldt has shown that, according to the "Fún-Tsaou," (a work on medicine and natural history, written four hundred years before the time of Columbus,) the Chinese suspended the magnetic needle by a fibre of silk, and found that it did not point directly towards the south, but deviated somewhat towards the southeast.

The directing property of the magnetic needle, and its use in navigation, became known in Europe at a considerably later period. It is mentioned, for the first time, by Are Frode, an Icelandic historian, who was born in 1068, according to the testimony of Snorro Sturleson, and who must have written his History of the Discovery of Iceland towards the end of the eleventh century. In this work he states, in the most unequivocal manner, that, in his time, the directing property of the magnetic stone was known. He also states that in

the year 868 Foke Vilgerdarson, the third discoverer of this island, a noted pirate, sailed from Rogaland, in Norway, in search of Iceland, or Gardarsholm, as it was then called, and took with him as pilots three ravens. To consecrate these to their important purpose, he instituted a grand sacrificial ceremony at Smoersund, when his ship was at anchor ready to sail; for, says Are Frode, the seamen in the northern regions were as yet unacquainted with the use of the leading stone. By the term leading stone the writer designated the natural magnet, which, in English, is still called loadstone or leadstone. It may, however, be presumed, from this form of expression, that in Frode's time the compass properly was not yet known, but that the natural magnet was suspended by a thread. According to the testimony of Hansteen, mention is made of the leidarstone or solar-stone, in the Sturlunga Saga. Gilbert, in his celebrated work "De Magnete," relates that, according to the report of Flavius Blondus, the Amalfitanes (Amalfis?) in Naples, first, about the year 1300, constructed and applied the mariner's compass, and this according to the direction of John Gioja, one of their fellow-citizens. He presumes, however, that more probably the knowledge of this compass had been brought from China to Italy, by Paul Venetus, about the year 1260. Gioja, of Amalfi, was, nevertheless, at least the first who placed the magnetic needle on a point, and divided the compass, according to the points of the horizon, into eight divisions.

That the mariner's compass, however, was known at an earlier period in the south of Europe, although in a rude form, is evident from a passage of a satirical poem, which was published by Guyot de Provins in 1203, and of which the original manuscript is still preserved in the royal (imperial) library at Paris. It is mentioned in this poem that the seaman easily finds the northern direction by the assistance of an ugly, black stone, called *mariniere*, and this even under a cloudy sky; that for this purpose it was only necessary to rub a needle with the stone, and then, attaching the former to a straw, allow it to swim on water, when it would point to the north. Cardinal Vitri, who lived about the year 1200, also makes mention of the magnetic needle in his history of Jerusalem, and remarks, moreover, that it is of inestimable value to mariners.

That the mariner's compass was known to northern nations is evident from the history of Norway, by Torfæus, in which it is stated therein that Yarl Stula was rewarded with a compass for a poem written on the death of the Swedish count Byrgeres. The directive force of the magnet is also distinctly alluded to in a letter to Peter Peregrinus de Marcourt, which was written towards the end of the thirteenth century. This letter was directed to "Sigerius de Foucancourt, a soldier in the service of magnetism," and contains a description of the magnet, of the means to find its poles, and of its peculiar attractive property in regard to iron, and finally proves that the extremity of the magnet which turns towards the north is attracted by the one that turns towards the south. One of the oldest treatises on magnetism is contained in a Latin manuscript of Peter Alsiger, which is found in the University library at Leyden, and was written in 1269. This manuscript, which seems to have been composed for the instruction of a friend, is divided into two parts, of which the first contains ten, and the second three, chapters. In the second chapter of the second part the mariner's compass is distinctly and perfectly described; and what is still more interesting, the author does not only mention the variation of the magnetic needle from the true north pole, but also gives an account of the accurate observations which he had made in regard to the amount of this deviation. "Observe well," says he, "that the ends of the magnet, and those of the needle rubbed with it, do not accurately turn toward the poles, but that the end which points toward the south inclines somewhat to the west, and the one pointing to the north in an equal proportion to the east." The magnitude of this deviation amounts, according to numerous observations, to five degrees.

The variation of the magnetic needle from the meridian, or its declination, as it is called, was known before the time of Columbus, to whom its discovery has been generally ascribed. His son, Ferdinand, in the biography of his father, written in Italian, and published at Venice, in 1571, relates that Columbus, on the 14th of September, (on the 13th according to Irving,) 1492, when he was at the distance of 200 leagues from the isle of Ferro, first observed the deviation of the magnetic needle, "a phenomenon, which," as the recorder says, "had never been observed before." Columbus found that at the dusk of evening the needle, instead of pointing towards the north star, deviated about half a point, viz., from five to six degrees towards the northwest, and on the following morning still more. Astonished at this discovery, he observed the needle for three days, and found that the deviation increased the further he advanced to the west. At first he did not call the attention of the crew to this phenomenon, well knowing how easily they might be excited to revolt. The sailors, however, soon became aware of the fact, and, on account of it, fell into the greatest consternation. It appeared to them that the very laws of nature were changing as they advanced on their adventurous career, and that they were entering into a new world governed by entirely unknown influences. They saw the compass losing its truthful character, and asked with alarm what would become of them without this guide on the trackless inhospitable ocean? Columbus had to tax all his ingenuity to appease their terror. He stated to them that the needle does not direct itself strictly towards the polar star, but towards another invisible point in the sky, and that the variation of the magnetic needle was not due to a change in the compass, but to the motion and the diurnal revolution of this celestial point around its pole. The confidence which the sailors had in the astronomical knowledge of Columbus gave weight to this explanation, and their excitement was consequently calmed. Although, as we have seen before, the deviation was known two hundred years previous to the voyage of Columbus, it is, however, evident from the facts just related that he made another discovery of not less importance, namely, that of the difference of the declination in different places of the earth.

We find more accurate notions of the declination of the magnetic needle, but these are as late as the middle of the seventeenth century. In the year 1541 the deviation of the needle from the meridian at Paris was found to be from seven to eight degrees to the east; in 1550 from eight to nine degrees; and, in 1580, eleven degrees and a half to the east. Norman, who first observed the deviations in London, found it to be 11¼ degrees in 1596, and Gellibrand, at the same place, in 1634, four degrees towards the east.

We have seen by what precedes that the magnetic needle does not point in all parts of the earth precisely to the geometric pole of the globe, and also that the amount of the deviation is not the same in all places. But it is important further to remark that a magnetic bar, free to move in every direction, will not remain stationary if placed in a horizontal position; on the contrary, in the northern hemisphere the north end of the bar will turn down towards the earth, and in the southern hemisphere the south end will assume a similar position. The bar will only remain horizontal in the region of the equator. The discovery of this important property, which is called the dip or inclination of the magnetic needle, has been generally ascribed to Robert Norman, (whose name has just been mentioned in connexion with the variation,) an Englishman, an experienced sailor, and, as William Gilbert calls him, an artist of genius. It is said the discovery was made by Norman in the year 1576, but, according to authentic documents, it was known as early as 1544 to George Hartmann, vicar of the church of St. Sebaldus, in Nuremberg. Hartmann was in correspondence with Albert, Duke of Prussia, one of those enlightened minds who recognized the importance of the sciences even at their early dawn. Their correspondence, commencing in 1541, was principally on scientific subjects, but the letter, which is

of the most interest to us at present, is dated March 4, 1544, and contains accurate descriptions of three magnetic discoveries which Hartmann had shown the year before, at Nuremberg, to Ferdinand, King of Bohemia, a brother of Charles V. This letter is found in the secret archives at Berlin, and was published by Mosor. Hartmann states in this that he had discovered that the extremity of the needle, which is intended to point to the north, must be rubbed with the end of the stone which points to the south, and that a needle so rubbed which has previously been accurately balanced so as to rest horizontally, will, after the magnetization, incline or dip at one end below the horizon. Further, that a large bar of iron placed vertically becomes so strongly magnetic as to repel with its lower end the northern point of a compass needle. This fact is best shown by using a large bar and a small needle.

The fact that rusted iron bars, which have remained for a long time in a vertical position, exhibit always more or less magnetism, was first observed in 1590 by Julius Cæsar, a surgeon, at Rimini, who observed that an iron rod, which had been placed for the support of the wall of the tower of the church of the Augustines, had become magnetic. Gassendi observed the same, in 1630, in an iron cross which had been thrown down by lightning from the church tower at Aix. He found that the rusted extremities of this cross had the qualities of the loadstone. When, about the year 1722, the iron cross which had adorned for several centuries the spire of the church tower at Delft was taken down for repair, the celebrated Loewenhoeck, on the suggestion of a stranger, as he says, obtained a piece of the iron from one of the laborers, but no influence was exhibited by it on the compass needle. Some time afterwards, however, the same laborer brought him a rusted piece from the foot of the vertical bar, which exhibited more power of attraction than the two natural magnets which Loewenhoeck possessed.

Whilst magnetism made but slow progress by incidental observations, it received suddenly a powerful impulse from the investigations of Dr. William Gilbert, of Colchester, England. This distinguished individual, who was physician to Queen Elizabeth, published in 1600 his "Dissertation on the Physiology of the Magnet," a work which not only contained everything known of magnetism and electricity up to that period, arranged in a truly scientific manner, but also a numerous and ingenious series of investigations on the subject by himself. He was the first who advanced the proposition that the earth itself acts, in all its parts, as a great magnet, in opposition to the opinions of those who, either with Olaus Magnus, supposed that there existed great magnetic mountains of such power that ships, in the construction of which iron had not been entirely omitted, would be attracted and held fast, or with those who placed the power of attraction in the sky, as, for instance, the astrologer, Lucas Gauricus, who supposed that a great magnet existed under the tail of Ursa Major, a constellation in the northern hemisphere to which all compass needles pointed. Gilbert logically refuted these and similar fanciful hypotheses, and substituted his own rational theory in their stead—a theory which, in its general principles, has been retained to the present time. He also attempted to explain, but with less success, the declination of the needle by ascribing magnetism merely to the solid parts of the earth, and not to the water, so that the needle would incline towards the continent, because a greater amount of magnetic power existed there.

It could, moreover, not escape the sagacity of a man like Gilbert, that the magnetic terminology, as he found it, was liable to great inconsistencies. Even in our days we are still accumstomed to call the end of the needle which points to the north its north pole, and the one pointing to the south its south pole. This form of expression is, nevertheless, incorrect, for if we admit that the earth is a great magnet, and that in the vicinity of the geographical north pole a magnetic north pole is situated, this north pole could only attract the south pole of another magnet, and consequently the end of the magnetic needle

which turns towards it should be called the south pole. In like manner the end of the needle which points to the south should be called its north pole. Gilbert objected to the use of this inconsistency, and introduced in its stead the correct appellation. He did not succeed, however, in abolishing the old terms, although physicists agreed with him, and even some of the more recent writers on this subject have adopted his forms of expression. This is the case with the French authors on magnetism, and some of the English physicists have endeavored to avoid the difficulty by using the term north end for the extremity of the needle which points to the north, and the south end for that which is directed to the south.

The fact was still unknown, even to Gilbert, that the deviation of the magnetic needle changes with time, and, upon the whole, there is but little trustworthy testimony to show to whom the discovery of the secular variation of the magnetic needle is to be attributed. Although observations made at Paris and London exhibit in different years a difference in the variations, the idea could not be seized upon at once that the needle changed its position from one year to another; on the contrary, it appears that the differences observed were considered as errors of the observations. Gellibrand, however, who observed the variation in 1634, in London, finding it different from that observed by Gunter in 1622, and that by Burrows in 1580, concluded that the deviation was variable, and therefore the discovery is generally ascribed to him. Although the French had observed as early as 1541, 1550, 1580, and 1603, in Paris, four different variations, and although Gunter, in London, had also found a deviation different from that of Burrows, the honor of the discovery cannot be ascribed to any of them, since the one who makes a discovery is he who first clearly perceives the essential particulars of the phenomena and gives an intelligible account of them; for this reason, and, indeed, with justice, the discovery of Uranus is ascribed to Herschel, although Flamstead had observed it nearly a hundred years before, but had mistaken it for a fixed star. The fact of the yearly variation of the magnetic needle was adopted and defended by Gassendi, in France, and was soon generally admitted, although it was thought at the time that the motion was regular, or that the north end of the needle moved every year an equal amount towards the west. It was, however, soon discovered that its progress was far from being regular, but it was still thought that the motion was so slow that the needle might be considered stationary at least for a few days. But this also proved to be incorrect when Father Guy Tuchart, in 1682, observed the deviation in the city of Louvo, in Siam, in presence of the King; he found it on four, and again on three successive days to assume different directions, either increasing or decreasing. The celebrated mechanist, Graham, in London, repeated these observations with better instruments in 1722, and discovered that the needle changes its position not only from day to day, but even from hour to hour; that, indeed, it does the same continually, and is, in fact, in a state of perpetual motion. Assessar Swedenborg, in his treatise on magnetism, expressed a doubt as to the correctness of these propositions, and asserted that they were based upon errors of observation. This induced the celebrated professor Celsius, at Upsala, to repeat the observations of Graham. As early as in 1740 he communicated a few results to the public, which showed the correctness of Graham's discovery.

Celsius was also the first who, in company with Hiarter, observed the remarkable and violent disturbances of the magnetic needle which accompany the appearance of the aurora borealis, and it was he who also first established the fact of the simultaneous motion of the needle at different places on the earth. He had induced Graham, in London, to make observations simultaneously with him, in order to ascertain whether the disturbances of the needle depends on local changes, or on those affecting large portions of the earth. After the death of Celsius, Olav Hiarter continued his observations and published the records of

the whole. From the comparison of these observations with those of Graham it was found that in Europe the needle is furthest to the east in the morning from eight to nine o'clock, and furthest to the west in the afternoon from one to two o'clock, when it again proceeds eastwardly until eight or nine o'clock in the evening, when it either remains stationary for a few hours, or makes a small movement of a few minutes back towards the west. During the night it generally moves somewhat towards the east, so that in the morning at eight o'clock it is found a little more to the eastward than in the evening.

About the year 1756, John Canton, in London, made observations on the daily deviations, or of the variations, as they were called, from which the result was deduced that the regular daily motion about the time of the summer solstice is nearly twice as great as at the winter solstice. In the first instance, it amounts to about $\frac{1}{4}$, in the latter to about $\frac{1}{8}$ of a degree. Canton endeavored to explain the daily western and the subsequent eastern variation of the needle by referring it to the influence of solar heat on the magnetism of the earth. He supposes, since magnetism is weakened by heat, that, if in the forenoon the sun warms the eastern parts of the earth, the needle will be more attracted towards the western parts, and in a similar manner in the afternoon, when the sun has weakened the western side, the greater influence of the eastern will draw the needle more towards that direction.

Before proceeding further in the exposition of this subject, we are obliged to take a step backwards and direct our attention to an individual who produced an epoch in the theory of the magnetism of the earth. We allude to Dr. Edmund Halley, of England, who in 1683 published his theory of terrestrial magnetism, which, in some particulars, still forms the basis of our present theories. He advanced the hypothesis that there were four magnetic poles, two in the vicinity of each geometrical pole of the earth, so that in different parts of the earth the needle always directs itself in such a manner that the influence of the nearest poles overcomes that of the more distant one. He further assumed that the pole which at that time was nearest to England was situated on the meridian of Cape Landsend, at the distance of seven degrees from the north geometrical pole, and that the other magnetic north pole was on the meridian of California, at the distance of 15 degrees from the north geometrical pole. He placed one of the two magnetic south poles 16 degrees from the geographical south pole, and 95 degrees west from London, and the other, the strongest of the four, at the distance of 20 degrees from the south pole, and 120 degrees west from London.

In order also to explain the successive variations of deviations, he advanced the remarkable hypothesis that our earth is a hollow sphere within which is a solid globe; that the two revolve around the same centre of gravity in nearly, though not in exactly the same time; and furthermore, that the solid globe is separated from the exterior hollow shell by a liquid medium. He also supposed that the internal globe, as well as the external shell, have each two magnetic poles, and that the changing deviation of the needle was produced by the want of perfect simultaneousness in the rotation of the two spheres. According to this hypothesis the magnetic poles of the external shell, while they do not coincide with the geometric poles of the same shell, always retained the same position, and, therefore, if the needle was only affected by them, the variation would always remain the same at the same place; but the needle being also acted upon by the magnetic poles of the interior globe, and as these slowly change their position relative to those of the exterior shell on account of the difference of velocity in the revolution of the two spheres, a change in the direction of the needle on all points of the earth's surface must be constantly going on.

Also after a complete rotation of the exterior within the interior sphere the variation must again become the same. This hypothesis created at the time a

great sensation, and in order to verify it and to discover from observations the law of the variation of the magnetic needle, Halley obtained, through the influence of King William, the command of a small vessel of the royal navy, in which he made two voyages in the years 1698 and 1699. He soon returned from the first voyage on account of his crew having fallen sick after passing the equator, and also on account of the mutiny of his lieutenant. In 1699 he sailed again and cruised in the Atlantic and Pacific oceans in various directions. From these voyages he gathered a sufficient number of observations to enable him to prepare his celebrated prospective chart of the variations of the magnetic needle.

On this chart he connected with continued lines all the places on the earth where similar and equal deviation of the needle had been observed, and thus produced a projection of what is called the lines of equal variation, or isogonic lines. These lines afford a ready means of presenting at once to the eye the totality of the phenomenon. They are also sometimes called Halley's lines, although, as may be inferred from a passage in Kircher, he was not the first who constructed such charts. Kircher, in fact, states, at page 443 of his *Nautica Magnetica,* that a Father Chr. Burrus had thought he had discovered a process by which longitude at sea might be determined, and had on account of it claimed a reward of 50,000 ducats from the King of Spain. His statement is as follows: On his voyage to India he observed, under the widely different meridians, the deviation of the magnetic needle, and collected also observations made by others. These observations, the number of which was not inconsiderable, he projected on a map, and then connected the places of equal variation by lines, which he called chalyboclitic lines. He asserted confidently that, by means of these lines, he could accurately determine the geographical longitude of a place by merely observing its magnetic variation. The insufficiency of this method was, however, recognized at the time. Gilbert made a similar proposal for determining longitude; but, instead of applying the variation, he thought to use the inclination or dip of the magnetic needle to obtain the object sought.

Euler, the great geometrician, also occupied himself with the theory of the magnetism of the earth, and endeavored to show that the hypothesis of Halley respecting four magnetic poles was unnecessary, and to prove from mathematical deduction that the assumption of the existence of two poles was sufficient; he determined the position of them for the year 1757. The north pole was beyond latitude 76° north, and longitude 96° west from Teneriffe; the south pole at latitude 58° south, and longitude 158° west.

In recent times a large number of the most accurate and valuable observations on the declination and inclination of the magnetic needle, and on the force of terrestrial magnetism in different parts of the earth, and especially in the neighborhood of the equator, have been made by Alexander von Humboldt during his travels. It was principally from these observations that the French physicist, Biot, endeavored to give an improved theory of the magnetism of the earth. He assumes in this theory that the magnetic poles are not situated on the earth's surface, but in its centre, and in close proximity to each other, and by means of a somewhat complicated mathematical process he succeeds in bringing the results of observations into apparent harmony with his theory.

But one of the most zealous promoters of our knowledge of the magnetism of the earth is Professor Christopher Hansteen, of Christiana, who, in 1817, published his work entitled "Investigations relative to the Magnetism of the Earth." An incident in the beginning of the year 1807 gave the first impulse to these investigations. Examining a physical globe constructed for the Cosmographical Society of Upsala, Hansteen found, at its south pole, an elliptic figure, designated by the name of "*magnetic polar region,*" and it was further inscribed on the globe that this magnetic polar region had been delineated by

Wilke from observations made by Captains Cork and Fourneaux. One focus of the ellipsis was designated as the stronger, the other as the weaker region. Hansteen was induced to compare these statements with the observations, and the comparison being satisfactory, he was led to investigate more thoroughly the theory of Halley, which, until then, he had looked upon as a wild speculation. The result of these investigations was that he became a convert to the theory of the existence of four movable magnetic poles.

In 1811 the Royal Danish Society of Sciences had offered the annual prize for the best answer to the question, "Whether it is necessary, in order to explain the magnetic phenomena of the earth, to admit the existence of several magnetic axes, or whether one is sufficient?" At the beginning of the following year Hansteen presented the greatest part of his work, as far as it was completed, and the society crowned his labors with its principal prize.

The most important part of Hansteen's work is that in which he treats of the number, the position, and the motion of the magnetic poles. From all the observations collected by him on the variations of the magnetic needle, he concludes that there are four points on the earth through which the lines of equal deviation pass, viz., a stronger and a weaker one in the vicinity of each geometric pole. Both the stronger poles, as well as the two weaker ones, are situated opposite to each other, as if they were extreme points of the same axes. All four have a regular rotation, the two northern ones from west to east, and the southern ones from east to west.

In order to elucidate the nature of the magnetism of the earth in each of its relations, Hansteen also undertook to make numerous observations, and even made a journey to Sibèria, in order to carry on his investigations within the region of greatest intensity of the magnetic phenomenon. This journey, besides directly enriching our knowledge of the magnetism of the earth with valuable results, had other consequences of great importance; it called the attention of the Russian government to this subject, and thus prepared the way for the labors of Alexander von Humboldt, at whose request the Emperor of Russia, with great liberality, ordered a number of magnetic observatories to be erected in his empire. Humboldt, immediately after his return from his travels in America, (1799, 1804,) had erected, in a garden at Berlin, an observatory, exclusively devoted to magnetism, and in which observations were made, often from four to six consecutive days, every half hour without interruption. The proposal of Humboldt, to erect similar observatories in other places of Germany, was not responded to partly on account of the political disturbances which were then visiting that country, partly because its celebrated citizen was intrusted with a mission from his government to France, and was thus hindered, for the time, in the pursuit of his favorite object. Arago commenced in 1818, at Paris, an exceedingly valuable series of magnetic observations, and by comparing them with such as were made simultaneously at Kasan, he confirmed the assertion of his friend Humboldt in regard to the importance and necessity of corresponding observations.

Humboldt returned to Germany in 1827, and established in the autumn of 1828 a continuous and regular series of observations. In consequence of his solicitation, the Imperial Academy of St. Petersburg and the curator of the University at Kasan, erected an observatory at St. Petersburg and Kasan, and under the protection of the chief of the mining corps, Count Canain, magnetic stations were established from the south of Russia through the whole of northern Asia. The Russian Academy sent George Fuss to Pekin, where he erected a magnetic observatory in the garden of the Greek convent, in which Kowanko made a continued series of observations corresponding with those of all the other stations. Admiral Greig also erected a magnetic observatory at Nicolajeff, in the Crimea; and, at the instance of Humboldt, a subterranean magnetic station was established under the supervision of Professor Reich, in the

mines at Freiberg, in Saxony, whilst Arago, at his own expense, had a declination compass placed in the interior of Mexico, at the height of 6,000 feet above the level of the sea. On the suggestion of Admiral Labord, the secretary of the navy of France directed the establishment of a magnetic observatory in 1836, at Reikiavik, in Iceland, and Humboldt sent instruments for an observatory to Havana.

In 1832 a new epoch commenced in the history of magnetic investigations; in that year Frederic Gauss, the renowned author of the general theory of the magnetism of the earth, as Humboldt calls him, erected in the observatory of Gottingen a set of instruments, constructed upon an entirely new principle. In 1834 this apparatus was transferred to a new observatory, expressly prepared for the purpose, and placed in charge of William Weber. After this, from Gottingen, as from a centre, was diffused over Germany, Sweden, and Italy, a spirit of magnetic observation with the improved methods and the instruments of Gauss. In 1836 four annual terms, each of twenty-four hours, were agreed upon by all the observers, during which a continued series of observations were to be simultaneously made, although the hours of these terms did not exactly correspond with those which Humboldt had proposed, yet they were unanimously adopted.

England had thus far taken no part in the general movement, although the celebrated English physicist, Sir David Brewster, made application to the government for the establishment of magnetic stations at different points of the British possessions, but it was here again, through the influence of Humboldt, that the desired result was obtained. He addressed a letter in April, 1836, to the Duke of Sussex, then president of the Royal Society in London, strongly recommending the establishment of permanent magnetic stations in Canada, at St. Helena, the Cape of Good Hope, on the Isle of France, Ceylon, and New Holland. In consequence of this letter, a committee of the Royal Society was appointed in order to examine and report upon the subject. It was proposed by this committee, in a letter to the government, not only to establish permanent magnetic observations, but also to equip ships for an expedition to the Antarctic ocean for the purpose of magnetic observations in that region.

(TO BE CONTINUED IN THE NEXT REPORT.)

ACCOUNT

OF SOME

RECENT RESEARCHES RELATIVE TO THE NEBULÆ.

BY PROFESSOR GAUTIER

Translated for the Smithsonian Institution from the *Archives des Sciences Physiques et Naturelles*, Geneva, 1862.

THERE is no part of the vast field of the astronomy of observation which is not at present the object of persevering explorations. I propose on this occasion to give a cursory view of those which relate to a widely extended and highly curious class of celestial objects, which was first made a subject of special study by the distinguished astronomers Herschel and Messier, and since by Lord Ross, by Fathers De Vico and Secchi, and by MM. Lamont, Lassell, and Bond; a subject which presents peculiar difficulties, and respecting which there remains much to be cleared up. I allude to the nebulæ, those small whitish patches, of feeble light, which the telescope reveals to us in great numbers in the heavens, and which powerful instruments enable us, for the most part, to recognize as assemblages of stars, situated at enormous distances from the earth.

In this rapid review I shall follow, in general, the order of dates, and I shall commence by saying a few words of a catalogue of the positions in the heavens of fifty-three nebulæ, the result of observations made at the observatory of Paris by M. Laugier, principally in 1848 and 1849, and by him presented to the Academy of Sciences of Paris at its sitting of December 12, 1853. This catalogue, published in the *Compte Rendu* of that sitting, gives with the precision of seconds of a degree the right ascensions and mean declinations of the centre or most brilliant point of those nebulæ to January 1, 1850, as well as the differences between these positions and those resulting from the catalogues of Herschel and Messier. It is a first attempt at precise determinations of the position of a certain number of nebulæ, undertaken with a view of serving to decide, in the sequel, the question whether these bodies are really situated beyond the fixed stars which are visible to us.

RESEARCHES RELATIVE TO THE NEBULA OF ORION.

M. Liapounoff, director of the observatory of Kazan, in the beginning of 1856 presented to the Academy of Sciences of Petersburgh, through the medium of M. W. Struve, a memoir on the great nebula of Orion, being the result of observations made for four years with an equatorial telescope of the power of that of Dorpat and a meridian circle of Repsold.* He has applied himself

* I know this memoir only from a very succinct mention of it at the end of the number of the *Monthly Notices* of the Astronomical Society of London for March 14, 1856, vol. xvi, p. 139. As I shall frequently have to cite this compilation, as well as that published at Altona by Dr. Peters under the title of *Astronomische Nachrichten*, I shall designate them respectively by their initial letters, M. N. and A. N.

to a very exact determination, by a process of triangulation, of the positions of all the stars which his instruments have enabled him to see in that nebula, and to a most careful delineation of all the parts of that remarkable celestial object, of which more than one chart had been already constructed, while assigning particular names to its several regions. M. Struve, in comparing the results of Liapounoff with those of Sir John Herschel, Lamont, and Bond, has expressed the opinion that this nebula must be subject to changes of form and relative brightness in its different parts.

M. Otto Struve has continued, at the observatory of Poulkova, the labors of M. Liapounoff, and has reported the first results of his researches in a communication, of the date of May 1, 1857, presented to the Astronomical Society by M. Airy, June 12 of the same year, and published in the seventeenth volume of the M. N., pp. 225–230.

In this, M. Struve begins by describing the variableness of the lustre of different small stars situated in the nebula of Orion—a variableness which he has verified as well by a comparison of his observations with those of other astronomers as by different observations of his own.* "The existence of so many variable stars," he continues, "in so limited a space of the central part of the most curious nebula of the heavens must naturally lead to the supposition that these phenomena are intimately connected with the mysterious nature of this body. * * Admitting that the rapid changes of light observed in these small stars, whether in the region called *Huygens* or in that called *Subnebulosa*, are connected with the nature of the nebula, it might be presumed that changes would be equally observed in the appearance of the nebula and in the distribution of the nebulous matter. But observations of this kind are subject to so many illusions, that we can scarce be sufficiently reserved in the conclusions drawn from them. I cannot think that the course commonly pursued by astronomers in this species of researches—the comparison, namely, with one another of graphic representations made at different epochs by different observers—ever conducts to results which can be regarded as indubitable. The optic power of the telescope, the transparency of the atmosphere, varying with different stations, the peculiarities of the observer's eye, the measure of skill and of experience in graphic representations of the kind—all this, joined with the influence of the imagination of the observer, forms obstacles which it will always be difficult to overcome in proceeding after this manner. It might perhaps be possible, by following this method for centuries, to discover progressive changes, if any exist; but those can never be thus verified which take place in short intervals of time. Now, the rapid variations of light in the stars may well cause us to expect similar, and perhaps periodical, variations in the appearances of the nebulous matter. It is therefore to rapid changes of this sort that we should particularly direct our attention, and we shall be better able to verify their existence by comparative observations on the degree of light and the forms of some prominent portions of the nebula than by representing it in its entireness. It was in this way that I endeavored to proceed during last winter, and the impression produced upon me was a strong one that, at different points, considerable changes occurred within the short period of my observations. I do not venture, however, to regard them as positive facts until they shall have been corroborated, especially by observers stationed in more favorable climates and provided with optical instrumentalities sufficient for the purpose." †

* I have heretofore had occasion to speak of this work of M. O. Struve, in a Notice on the Stars of Variable Brightness, published in the numbers of the *Bibliotheque Universelle* (*Archives*, vol. xxxvi, pp. 5–89) for September and October, 1857. M. Otto Struve has recently succeeded his father in the direction of the great Russian observatory of Poulkova.

† The memoir of M. O. Struve on this subject has been published, I believe, in vol. ii of a collection entitled, *Melanges Mathematiques et Astronomiques.*

M. O. Struve proceeds to mention in detail four parts of the nebula of Orion in which he perceived most distinctly, in an interval of some months, changes of form or of the degree of light. The first is a bay, extending from the *straits of Le Gentil* in the direction of the trapezium of stars situated towards the middle of the nebula. This bay appeared to him at one time altogether obscure, like the straits; at another, full of nebulosity, and little inferior in brightness to the surrounding portions of the region of Huygens. Dr. Lamont first delineated this bay, which has never been seen by Sir John Herschel. The second is a *nebulous bridge*, which crosses the *great straits*, with a point of concentrated light about midway. M. Struve saw it in winter, sometimes as represented by Herschel, sometimes as by Liapounoff, with much greater concentration of light, but always much more extended than in the representations of these astronomers, and closely approaching the southern limit of the great strait. Very faint traces of it are indicated by M. Lamont, while Professor Bond did not see it at all. The third is a nebulosity surrounding star 75 of Herschel's catalogue, which appeared to M. Struve to be subject to great changes of brightness. Lastly, the fourth part is a sort of narrow canal, uniting in a right line the obscure space situated around the stars 76, 80, and 84, of Herschel's catalogue, with the north side of the *great strait*, near the exterior extremity of the bridge before mentioned. The canal, which has not been represented by any other observer, was distinctly seen by M. Struve March 24, 1857, while on other occasions he has not perceived the least trace of it.

This astronomer, in closing his communication, adds, that the general impression resulting from his observations is to the effect that the central part of the nebula of Orion is in a state of continual change of brightness as regards many of its portions. In those cases where the images were most distinct, their appearance did not seem entirely uniform from night to night. These changes in the degree of light cannot, however, be perceived in the greater number of cases without instruments of considerable optical power; and he does not think that achromatic telescopes of less than ten inches opening can serve to verify them, except under atmospheric conditions extraordinarily favorable.

The twenty-second volume of the M. N. (pp. 203–207) contains the analysis of another memoir relating to the same nebula. It was communicated to the Astronomical Society, May 10, 1861, by Professor George Bond, who has succeeded his father in the direction of the observatory of Harvard College, at Cambridge, near Boston. The paper bears for its title, *On the spiral structure of the great nebula of Orion.*

Professor Bond the father, in a memoir published in 1848, had already remarked that the light of this nebula seemed to present a radiated appearance on its southern side, starting from the neighborhood of the trapezium of stars situated towards its middle. Professor G. Bond has undertaken, since 1857, to form a catalogue of the stars comprised in a square of forty minutes to the side, having θ of Orion for its centre. He selected one hundred and twenty-one bright stars as guiding points to which to refer the smaller stars, of too feeble light, for the most part, to remain visible under a strong illumination of the micrometric threads. In a first sheet he has arranged two hundred and sixty-two stars, and then subdivided the same surface into four charts, finally reunited into a single one. The form and arrangement of the elongated luminous tufts, alternating with the more obscure spaces stretching from the neighborhood of the trapezium, have been determined by two independent procedures, the nebula being first delineated as a bright object on a dark ground, and then as a dark object on a white ground.

I cannot enter here into the descriptive details given in the analysis of Prof. Bond's memoir, and I shall confine myself to a report of its conclusion. The general aspect of the greater part of the nebulæ of Orion is an assemblage of tufts or curvilinear pencils of luminous matter, emanating from bright masses

near the trapezium, extending towards the south, on each side of an axis passing by the apex of the region called Huygens, of which the angle of position is in the neighborhood of 180°. Some twenty of these circumvolutions have been distinctly traced, whilst others, producing the same impression, are too faint or too complicated to be described with precision. We may class, then, according to Prof. Bond, the nebulæ of Orion among the *spiral nebulæ*, such as they were, for the first time, described by Lord Ross, with the aid of his great reflecting telescope. The nebulæ No. 51, of the catalogue of Messier, was the first in which he discovered this spiral conformation, which had escaped both the astronomers Herschel.

Prof. Bond has observed that, in a great number of cases, the masses of nebulous matter are associated with stars, frequently under the form of small tufts extending from their southern side. He cites two remarkable instances where there is a deficit of luminous matter near stars of considerable brilliancy; the first, in reference to the trapezium itself, whose obscure centre has been remarked by sundry observers; the other, to the star *Iota* of Orion. These peculiarities appear to Prof. Bond to be favorable to the supposition of a physical association of the stars with the nebulæ. The existence of an arrangement in a spiral form of the parts which compose it accords with the idea of a stellar constitution; for among the objects which present this peculiarity of form are found not only nebulæ resolvable into stars, but masses of stars properly so called, such, for instance, as the grand mass of stars of the constellation Hercules, where the exterior stars have evidently a curvilinear arrangement.

OTHER FACTS RELATING TO THE NEBULÆ.

M. Norman Pogson, whilst at the observatory of Dr. Lee, at Hartwell, in 1860, witnessed a change in the nebulæ, or mass of stars, No. 80 of the catalogue of Messier, situated in the constellation of the Scorpion, and very close to a pair of variable stars R and S of the Scorpion, which have been observed by M. Chacornac since 1853. The 9th of May this nebulæ had its usual aspect, without any stellar appearance, and the 28th of the same month Mr. Pogson saw therein a star of the 7th or 8th magnitude, which has been also observed since the 21st of May by MM. Luther and Auwers at Köningsberg, and which the latter have estimated to be of something more than the 7th magnitude. The 10th of June following, with a magnifying power of 66, the stellar appearance had nearly passed away, but the nebulæ had a greater brilliancy than usual, with a clearly marked central condensation. M. Pogson does not think that this variation can be attributed to a change in the nebulæ itself, but he regards as singular that a new variable star, the third comprised in the same field of vision, should be found exactly situated between the earth and that nebulæ. This observation has been published in the twenty-first volume of the M. N., p. 32.

M. Chacornac has observed quite recently, with M. Foucault's great reflecting telescope of plated glass, so adapted as to procure a great degree of enlargement, the annular nebulæ of the Lyre, and he has ascertained that it is in reality resolvable into a mass of very small stars, closely crowded together, the brightest of them occupying the extremities of the small diameter. This nebulæ, in an examination of several nights, presented to him the appearance of a hollow cylinder, seen in a direction nearly parallel to its axis; and its centre, as Lord Ross describes it, is veiled by a curtain of nebulous matter, which converts itself into a somewhat thin stratum of little stars. M. Chacornac adds, in a communication to Dr. Peters on this subject, dated Paris, 9th June, 1862, and published in No. 1368 of the A. N., that when the view is screened from all interfering light, the scintillation of this multitude of luminous points, occupying a large portion of the surface of the retina, produces a sort of giddiness which is quite curious.

I pass now to the labors of M. d'Arrest, relative to the nebulæ. This astronomer had begun to occupy himself with this subject while he was still attached to the observatory of Leipsic, and, since 1857, has published in the collection of the memoirs of the Royal Society of Saxe the result of his first observations of 230 nebulæ, made with a double annular micrometer, of Frauenhofer's construction, applied to a telescope of 52 lines opening and 6 feet focal length. Prof. d'Arrest* is at present director of the observatory of Copenhagen, and he has continued, since the month of September, 1861, his observations of the nebulæ, with a large achromatic telescope, of 11 inches opening and 16 feet focal length, the optic power of which he estimates to be intermediate between that of Herschel's 20 feet reflecting telescope, and that of the telescope of the same kind with which Lassell likewise has observed the nebulæ from 1852 to 1854. The telescope of Copenhagen has enabled M. d'Arrest not only to recognize all the nebulæ of Herschel, but to discover more than a hundred new ones among 776 observed in eight months. He has been enabled also, under favorable circumstances and with some difficulty, to see certain nebulæ indicated by Lassell.

M. d'Arrest, making his observations alone, soon perceived that he could scarcely combine the observation of celestial objects of very feeble light with the microscopic reading of the circles of his instrument. It follows that his new catalogue will not assign, with all the precision attainable, the absolute position of each object on the celestial sphere. This position is only given to the minute of a degree in right ascension and in declination ; but as the nebulæ are very carefully compared with the neighboring small stars by the help of annular and thread micrometers, we shall thus have competent means for ascertaining with precision their proper movements in respect to those stars, which constitutes one of the principal aims of the researches of M. d'Arrest. This astronomer has published, in No. 1366 of the A. N., an interesting notice, dated 20th May, 1862, of his later labors; and from this I shall extract some details, tending to complete those which precede.

VARIABILITY OF THE BRIGHTNESS OF THE NEBULÆ.

M. d'Arrest considers as well established one of the results of the great labor of Argelander, in which has originated his new catalogue of stars, namely, that, of 50,000 stars already well recognized, there exists but a small number of which the brightness is periodically variable; and he believes the same may be affirmed, though with less certainty, to be very nearly the case with the nebulæ.

Sir W. Herschel had subdivided the nebulæ into three classes, with reference to their relative degree of brightness. M. d'Arrest has found a great many instances in which the nebulæ, as at first classed by Herschel, must now be assigned by one or even two units a new place in the classification. Herschel himself had, in the course of some years, changed several of his appreciations. But in view of the great diversity of atmospheric influences in humid climates, bearing upon observations of this kind, M. d'Arrest thinks, like M. Otto Struve, that it is impossible to be too circumspect in regard to the conclusions to be deduced from variabilities of this nature. He instances, however, a small number of cases in which some degree of variability has been positively ascertained.

The first of these cases is that resulting from the observations of M. O. Struve on the nebula of Orion before spoken of. The observations of this nebula recently made, at different times and in favorable nights, by M. d'Arrest, with his large telescope, have confirmed those of M. Struve, particularly as regards

* See M. N., vol. xvii, p. 48.

the *bridge* over the *great strait*, which, last winter, was sometimes distinctly visible at Copenhagen, presenting the appearance assigned it by M. Lassell.

The second case of well-established variability is the almost total disappearance of a small and faint nebulæ discovered by M. Hind, October 11, 1852, in the constellation Taurus, recognized by other astronomers, and in the beginning of 1856 still readily perceptible with a telescope of six feet focal length. Two years later it was no longer to be seen, except with great difficulty, in the heliometer of the observatory of Königsberg. It was invisible October 3, 1861, with the great telescope of Copenhagen. M. Chacornac, with the new telescope of M. Foucault, and M. Lassell, at Malta, with his reflecting telescope of four feet diameter, sought for it in vain in 1862, though it was still to be seen with the great achromatic telescope of Poulkova.

A curious circumstance, connected with the great diminution of the brightness of this nebula, is that this diminution has coincided with that of a small star which presented itself almost in contact with the nebula. M. Argelander estimated the magnitude of this star, in 1852, at 9.4. It was of not more than the tenth magnitude in 1858, of the eleventh in 1861, and of the thirteenth or fourteenth in 1862.

Sir John Herschel believed that he had recently detected another instance of the disappearance of a nebula from not having found inscribed in the first catalogue of M. d'Arrest a very faint nebula, noticed by Sir W. Herschel, near two others in the constellation of Berenice's hair. But M. Chacornac has ascertained, with the new telescope of M. Foucault, that this feeble nebula is still plainly visible, and M. d'Arrest has also observed it with his great telescope. The latter astronomer further cites a small number of cases where there may have been variability of brightness, or even disappearance of nebulæ; but these instances are not as well established as that of the nebula of M. Hind.

DOUBLE NEBULÆ.

Sir John Herschel has remarked in his important memoir on the nebulæ, published in the *Philosophical Transactions* for 1833, p. 502, that the number of nebulæ physically connected with one another is probably more considerable, relatively to the total number of the nebulæ, than is that of double stars among the fixed stars.* Admitting a mutual distance of five minutes of a degree to be the greatest for the double nebulæ, M. d'Arrest even now enumerates about fifty comprehended within this limit, and is of opinion that there may be two or three hundred of them among the whole number of some 3,000 nebulæ discernible in our heavens.† So considerable a proportion of double nebulæ justifies the presumption that there is a real connexion in these groups, and their aspect confirms this idea, particularly in the case where unusual forms present themselves at once in two equal exemplifications. Sir W. Herschel seems not to have had an idea of this physical connexion between the nebulæ, but Sir John distinctly speaks of it on more than one occasion. It can scarcely be doubted that at some future period astronomers will be called on to calculate the orbits of the double nebulæ.

M. d'Arrest mentions some particular cases of this sort of nebulæ, of which one is triple. There is as yet but one recognized, where, on comparing the distances and respective positions of the two nebulæ of the same group observed in 1785, 1827, and 1862, considerable changes have been noticed, which seem to indicate a movement of revolution of the one around the other. This double

* A short analysis of this admirable paper of Sir J. Herschel, accompanied with a plate, is given in the issues of the *Bibliotheque Universelle* for June and July, 1834.

† M. d'Arrest has quite recently published, in No. 1369 of the A. N., a catalogue, for the commencement of 1861, of the positions and aspect of fifty double nebulæ which he has already recognized, and of which a dozen are new ones.

and particularly interesting nebula is situated at 109° 12′ of right ascension, and 29° 45′ of north declination. M. Lassel has represented it in Fig. 9 of Plate XI, accompanying his memoir, published in vol. xxiii of the quarto collection of the Astronomical Society of London. The two components of the nebula are very distinct, though their mutual distance is at present but 28 seconds; but they are difficult to be seen when the threads of the micrometer are illuminated. A very small star is found between the two, exactly in the same place where M. Lassel observed it ten years ago. M. d'Arrest will take occasion, when his labors on this subject are completed, to cite some other analogous cases of change of relative position in double nebulæ. His presumption, in the mean time, from what he has been able thus far to discern, is, that there will not be found in any of these groups of nebulæ as short periods of revolution as those which have been verified in the case of some of the double stars.

Finally, M. d'Arrest reports a small number of cases where, by comparing a nebula with some small star near it, and repeating this comparison at the end of a certain time, he has been able to verify slight differences of distance or of position, which might indicate a proper movement in one or the other of those bodies.

I here terminate this short review, in which I have been able to give but a rapid glance at the present state of observation in respect to one of the most difficult and least advanced parts of astronomical science.*

Post scriptum.—M. d'Arrest has just announced, in No. 1378 of the A. N., that he has recognized in the constellation Taurus the existence of a second nebula of variable brightness.

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* I ought here to correct an error, pointed out to me by Dr. Hirsch, which I committed in my notice on the observatory of Neufchatel, inserted in the number of the *Archives* for last July, volume xiv, p. 224. It is not M. Hirsch, but Professor Kopp, of Neufchatel, who forms part of the meteorological commission instituted by the Helvetic Society of Natural Science.

FIGURE OF THE EARTH.

BY SR. MIGUEL MERINO.

Anuario del Real Observatorio de Madrid; cuarto año; 1863.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

In an article inserted in our Annual for 1862, under the same title with the present, we proposed, as our nearly exclusive object, to present, in an elementary manner, the result of the investigations heretofore made to determine the form and volume of the earth, apart from historical notices, numerical details, and, in a word, whatever might embarrass the course of the reasoning, or distract the attention of our readers. Thus conceived and compiled, that first article was for the most part dry, as regards results, and incomplete under various aspects. Dry, inasmuch as the mind takes less pleasure in the final solution of a problem than in the survey of the means and computations employed to overcome, one after another, the difficulties which beset it; and incomplete, because without numbers there is in the physical sciences no precise solution, such as shall leave the mind tranquil and satisfied. To supply our intentional omission is the design of the present pages, in which, assuming the substance of our former article to be known, we shall consider, successively, and under the new point of view just indicated, the three following points:

First. In what manner the human understanding, acted upon by the immediate testimony of the senses, acquired, after a long uncertainty, a clear idea of the roundness and rotation of the earth.

Second. By what means that first idea, founded on a somewhat superficial examination, became confirmed, and, at the same time, modified in some of the details by the actual and direct measurement of our globe.

Third. The present state of the question, briefly summed up in certain numerical tables.

I.

In considering the progress of astronomy we must distinguish two epochs of quite different character—one very remote, and only known to us by vague and confused tradition, which has often undergone a strained interpretation; the other nearer to our own times, whose history has been consigned to unequivocal and imperishable monuments. In the opinion of certain authors possessed of erudition and talent, and doubtless sincere in their belief, but led astray possibly by the excess of their imagination, the ancient people of central Asia, the Chinese, Indians, Assyrians, and Chaldeans, as well as the Egyptians, enjoyed a civilization superior to the modern, cultivated the sciences, and possessed, particularly, a knowledge of celestial phenomena, to which our present astronomers cannot yet pretend. In the possible case that this were so, although the mind instinctively revolts from believing it; that astronomy flourished at a period of which history preserves no distinct traces, and that all which we know to-

day respecting the form of the earth and its relations with the other bodies of our system was known many ages before that in which we live; granting. moreover, that in view of the constant and great vicissitudes to which the world is subject, where the events of to-day so readily and radically efface the most momentous memories of yesterday, we are left without any positive grounds for roundly denying the above assertions, yet what imports it to us whether the primitive people of Asia were more enlightened than those of modern Europe, if there remain only incomplete traces of their knowledge—if their science has disappeared or been transmitted only when the modern had secured new foundations, assuredly not less solid than the ancient? If we concede at once that those people had ascertained the roundness of the earth, whether from the experience acquired in their emigrations and their warlike and commercial expeditions, or else from a species of intuition; that from the demonstrated fact they ascended to the producing cause, and that, not content with a knowledge of the form, they had sought and succeeded in determining the dimensions of the globe, what advantage have the moderns derived from all this? In what respect have these problematical antecedents served to enlighten us with reference to the questions with which we are engaged? This is to us the point of interest, and it is this which we should first of all endeavor to make plain.

In his heroic poems Homer brings together all the cosmographic and geographic ideas of his age and of the people to whom he belonged—a people fitted, beyond all then known, for the cultivation of the sciences, distinguished by their lively and penetrating imagination, and inhabiting a country in all respects the most favorably situated for observation. And yet Homer, minute and exact as he is in the description of the scene on which his heroes moved, supposes the earth to be a plane, and bounded in all directions by the waters of the ocean; places in the middle of it Greece, and particularly the Thessalian Olympus; establishes, on the mysterious limits of the horizon, pillows which serve as a support for the skies; pictures Tartarus, the abode of the enemies of the gods, at a great depth beneath the surface; and beyond the dim confines of earth imagines *chaos*, or immensity, a confused mixture of life and vacuity, an abyss where exist, without order, all the elements of Tartarus, earth, and heaven. Here we have the point of departure for our existing knowledge respecting the form of the earth and the constitution of the celestial vault; and is there here anything which reveals the profound research, whether certain or problematical, of the pristine races? Have we here, indeed, anything more than the primitive ideas, which the spectacle of nature wakens in the breast of every one moderately endowed with an inquiring spirit, dressed in the colors of a glowing imagination, but betraying the incapacity to discover the truth through the mists which envelope it?

The voyages of the Phenicians, though conducted with less timidity than those of the cotemporary Greeks, yet with a prudence and caution indicative of no transmitted knowledge, open the door to wider investigation, to juster ideas of the figure of the earth, and lead, by a more certain, at least more expeditious path, to the discovery of the truth. Till this epoch history presents to us each people shut up within the narrow limits which nature had marked for it, here separated from the rest by mountain chains, there by tempestuous seas. The dwellers of Tyre and Sidon are the first to venture habitually on distant voyages in search of new lands, of foreign productions, of the objects of luxury and affluence, which were wanting at home. They visit, one by one, all the islands of the Mediterranean, coast along the north of Africa, founding colonies wherever suitable; and, without recoiling before the dreaded straits of Gades, launch into the ocean and establish the principal seats of their commerce on the smiling shores of Betica. And while advancing on the west to points never before reached, this commercial people unite the fleets of their King Hiram with those of Solomon to explore the coasts of the Red and of the

Erythrean or Indian seas; while still later, as some historians maintain, their boldness reaches such a point that they navigate the shores of Africa by the east, double the Cape of Good Hope, afterwards long forgotten, and regain their country at the end of three years by the before-mentioned straits of Gades, or Gibraltar.*

The Carthaginians, possessing the same enterprising and mercantile genius with their ancestors of Phenicia, and benefiting by the experience of the latter, projected still more important, if not more daring, expeditions. Hanno, with a numerous fleet, traces the western coast of Africa and attains the mouth of the river Senegal, while Himilco, sailing in the opposite direction, stops not short of England, where he loads his vessels with the coveted metal stored in that region.

Similar expeditions, made with ever-increasing frequency and boldness, such as the voyage of Colœus of Samos, which extended to the entrance of the Atlantic, and so strongly excited the curiosity of the Greeks, and the much later one of Pytheas† of Marseilles, who advanced as far as the Feroe islands, and even entered the Baltic, although they might be undertaken solely with the views of adventure or cupidity, could not but be conducive to the progress of astronomy and its kindred sciences, as well in regard to the preliminaries they required, as the observations and notices collected in these protracted wanderings. However closely attracted to the land by necessity or interest, can we suppose that these early navigators did not often lift their eyes to contemplate the celestial vault, induced as well by the requirements of safety as by the curiosity inherent in man of seeing and learning something new? In this way the old impressions that the earth was plane and undefined, that the stars, quenched in the sea, were again kindled at their rising, and others of the same kind, would necessarily give way, not alone in the conceptions of the thoughtful, but in the opinion of the vulgar, and be replaced by ideas more creditable to human sagacity, and conformable to the truth and simplicity of nature. To this result would conduce, indirectly but still effectually, the travels undertaken by land, whether towards the north in search of amber, furs and materials of construction, towards the east for ivory and spices, or towards the west for metals. The wars among nations would also promote this result, as necessarily tending to a mixture of races, and a comparison of conflicting ideas. Among influences of this kind we may especially distinguish the expedition of Alexander, at once enlarging beyond example the limits of the known world, and bringing into propitious coincidence a vast material and a most favorable conjuncture of circumstances for new and fruitful meditation; the conquests of the Romans, extend into one almost all the nations of the known world, and attracting to the common centre whatever that world contained which could minister to an unbounded love of ostentation and luxury; the Gothic irruption, covering the world with ruins from which the germs of knowledge might spring with a new and more vigorous life; and the subsequent appearance of the Saracens,

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* This voyage of circumnavigation, of which Herodotus speaks as having been undertaken about the beginning of the sixth century before our era, and at the instance and direction of Necos, King of Egypt, has always met with warm asserters and oppugners. To us the arguments of the latter seem to have the most weight, though amongst the former appears the learned and judicious Cesar Cantu. In so disputable a matter, doubtless, the reader need not resign himself blindly to the opinion of any one; but, for our present purpose, it is sufficient to know, that if such a voyage was really performed, it led to no results worthy, from their curiosity or importance, to be transmitted to modern times. As regards other ancient voyages around Africa, there are still stronger reasons for discrediting them than that attributed to the Phenicians.

† The reality of the voyage of Pytheas, to the west and north of Europe, is generally admitted, but the descriptions given by him of the lands and seas he visited are regarded as exaggerated, as they are certainly in many points obscure, even when we concede their foundation in fact.

endowed with a special culture and heirs of the ancient civilization of the East, upon the theatre where modern civilization was undergoing its definite development. This series of momentous events, we repeat, in proportion as it conveyed to each people the traditions and impressions of the rest, as it brought into contact, beneath another climate and sky, under natural conditions essentially different from these in which they had before lived, the natives of regions most widely remote from one another, could not but prompt human reason to discard the trivial ideas which it had cherished and insensibly adopt others more in harmony with the truth of nature. And this, be it observed, without the intervention of ancient science, lost or at least forgotten amidst the convulsions which had swept away its cultivators, and solely by an immediate effect of the events which, at the epoch of regeneration referred to, constantly modified the state of societies.

The incursions of the northern hordes having at last ceased, the present nationalities began to take shape; and if the systematic cultivation of the sciences was not yet to be expected, at least a delight in their study began to dawn. Nor did eventual circumstances, and such as might have appeared extraneous, cease to stimulate the taste for voyages and discoveries. As the occupation of the south and west of Europe by their warlike predecessors opposed an insuperable barrier to the progress, in that direction, of the Scandinavians or Normans who brought up the rear of the Asiatic migration, these established themselves permanently on the shores of the Baltic, and from thence, impelled by their roving and hardy genius, explored the northern islands and continents, the archipelagoes of Shetland and Feroe, Iceland, and, in the tenth and two succeeding centuries, the inhospitable coasts of Greenland, and those, somewhat more fertile, of Vineland, the present Labrador.* Meanwhile there arises in Asia a formidable empire, whose limits expand with astonishing rapidity from the seas of India to the frontiers of Europe, giving rise to the dread of a new invasion of destructive races; yet its service as a counterpoise to the Saracenic power, not less formidable on another side, is appreciated, and pontiffs and kings send embassies, sometimes to propitiate the redoubtable successors of Genghis-Khan, sometimes to solicit help from Tartar and Mogul princes, at times simply in sign of admiration and respect. At their return from these distant scenes, observers like Ascelin, Carpini, Rubruquis, Polo, Sotomayor, and Clavijo, whether sent as ambassadors or led thither by inclination, communicate their impressions and adventures without reserve, and awaken in all

* The following is a recapitulation of the later discoveries referred to in the text. They may be found more particularly described in the eighteenth book of Malte Brun's Geography, and in the notes to the fourteenth book of Cesar Cantu's Universal History.

About the middle of the ninth century Iceland was discovered, and before the end of the century a numerous colony of northmen was established in that island. In 986, among other colonists, one called Eric the Red, having been banished from Iceland, takes refuge in Greenland. Biörn, son of Eriulph, one of the companions of Eric, desirous of joining his father, freighted a ship and directed his course towards Greenland, but wandering for some time in those seas, got a sight of new coasts other than that which he was seeking. In the same ship with Biörn, Leif Ericson, son of Eric the Red, set sail from Greenland in the year 1000, and visited in succession a sterile, rocky and snow-covered coast, (Helluland;) another level, hoar with frost and well wooded, (Markland;) and a third, which abounded in vines, (Vineland.) Thorwald and Thorstein, brothers of Leif, prosecuted, with no successful result, the exploration of these lands, as did others of the same race. And though commerce and communication between Iceland, Greenland and the parts last mentioned, continued for a considerable length of time, they underwent many alternations, and proved of no real importance to geography. Judging from the descriptions given, as well of the lands as of the celestial phenomena which were observed, Markland seems to correspond to Nova Scotia, and Vineland to the region about Cape Cod, as far south as latitude 41°. If the documents published by the Society of Northern Antiquaries may be relied on—and it is not our province to controvert them—Columbus made no true discovery; but how the Icelandic adventurers came to stop midway, and allowed the intrepid Genoese to snatch from them the domain of a world, is a phenomenon difficult to explain, and, in our opinion, more discreditable than otherwise to those in whose honor it is cited.

minds more competent conceptions of the form and size of the earth, and of the diversity of climates, than could otherwise have been attained. Thus, by means the most indirect, the limits of the world were extended, many obscure spaces of the earth brought to light, and the minds of men prepared for greater and more decisive discoveries.

We have now arrived at the first half of the fifteenth century. Portugal is a prosperous kingdom, without near enemies to combat, and possessed of a vitality which refuses to confine itself within the territorial frontier; it claims a wider field, and enterprises more worthy of the national spirit. With this spirit the geographical position of Portugal at one of the extremities of the ancient world, in front of that world which now awaits discovery, concurs to make it the point of departure for the great maritime expeditions of the age. Its princes, too, second opportunely the impulse, as well by their patronage of science and its cultivators as by a steady faith and interest in all enterprises calculated to enhance the name and importance of their country. Under the protection of Prince Henry, the Portuguese navigators explore and take possession of the archipelagoes of Azores, Madeira, and Cape Verde, and double Cape Bojador, so long the terminus of the African coast, thus penetrating into the vast Gulf of Guinea. Still later, in 1486, Bartholomew Diaz reaches the southernmost extremity of Africa, to which he gives the name of the *Cape of Storms*, a name soon changed by King John II into the more propitious one of *Good Hope;* and, finally, Vasco de Gama, passing, in 1497, beyond this formidable promontory, and turning his prow in an opposite direction to that of the supposed Phenician navigators of a remote age, points out to his adventurous cotemporaries the maritime route to India and China, immense regions till then only known through vague and inexact tradition.

It seemed impossible that the ardor of the Portuguese for distant and hazardous exploration could be surpassed by any other country, and that still more important successes were in reserve for a different people. And yet this seeming impossibility was realized in a manner the most simple and natural, and with means the most limited imaginable. The genius and perseverance of an obscure and ill-understood mariner having met, though after long struggles, with support and countenance in the faith and enthusiasm of a queen, Columbus was enabled to launch his three frail caravels, manned by a handful of Spaniards, upon the broad Atlantic; there, leaving the Portuguese to contend with the dangers of the African coasts, and disregarding the circuitous and unprofitable track pursued by the Scandinavian adventurers, he directed the course of his vessels first south, and then constantly west, until he reached the archipelago of the Antilles, the gate of a new world resplendent with beauty, which seemed at that moment to ascend from the bosom of the seas.

Among the multitude of daring navigators who followed Columbus in the work of western exploration we may distinguish Magallanes, a Portuguese in the service of Spain, for the importance of the results attending his enterprise. After the discoveries of Columbus and De Gama, it still remained to be ascertained what separated, and at how wide an interval, the two continents to which they had led the way. There existed, as Balboa had descried, in 1513, from the Isthmus of Darien, a vast sea, but of its extent no conception had been formed, and yet Magallanes, not more enlightened on this point than previous explorers, proposed to traverse it. He sailed from Spain in September, 1519, passed the next year through the difficult straits which bear his name, and perished in the Philippine islands, after having overcome the chief difficulty of his undertaking. His second in command, Elcano, a Biscayan by birth, and not less resolute than the chief he had lost, still continued his course westwardly, and finally regained his country in a direction opposite to that by which he had departed. The sphericity of the earth, already recognized by reflecting minds, and gradually revealed by the discoveries which have been here briefly re-

traced, was now for the first time practically shown; to the rude mariner, as to the astronomer, the limitation of our globe in all directions, and its isolation in space, were from this date evident; and to the abstract but little diffused methods of geometry was now added a new means for forming an idea of the dimensions of the earth. Elcano, in effect, though encountered by many unexpected obstacles, had performed, in little more than three years and three months, a complete voyage of circumnavigation.

From the memorable epoch referred to, geographical discoveries have succeeded one another with greater rapidity than ever, the earth has been explored in all directions, the width of the seas calculated, and the surface of the continents measured; but all these labors, however vast their importance, have been those of detail, and have added no new idea to the results of the bold navigation performed in the fifteenth and beginning of the sixteenth centuries, as respects the general form and approximate dimensions of the planet we inhabit. To these last dates must be referred, if not the first clear conception, the definitive verification of the nearly spherical figure of the earth.

Looking back, however, it must be conceded that neither the voyages of Columbus nor Magallanes were absolutely necessary for the demonstration of the spherical figure of the earth, since this fact might have been deduced with sufficient clearness from geographical principles already verified; from the delusion indulged by every nation that its own territory was central, as regarded the rest of the earth; from the general and changeable aspect of the heavens upon every change of country; from the apparent sphericity of the sun, and especially that of the moon, still more conspicuous through the succession of its phases, and from the circular outline of the earth's shadow during the eclipses of the lunar planet. But all these indications of the limitation and roundness of the earth, however conclusive for reflecting and studious minds, would have carried no conviction to the generality of mankind, without the incontestable support of those other proofs which might be called material or tangible. With what obstacles did Columbus meet before finding himself intrusted with three frail vessels—how much incredulity in all countries, even to the extent of being charged with madness—and for what? Columbus said: "The Portuguese seek the gold and spices of India by steering towards the east; and I, who cherish the persuasion that the earth is round, propose to trace a more expeditious route by reaching the same point in an opposite direction." Had there been many who at that time held the doctrine of the earth's sphericity, no one would have treated so logical and obvious a thought as extravagant; nor would Columbus have been indebted to the noble instinct of a woman for the successful issue of his enterprise if modern society had inherited from the ancient that vast store of science attributed to the latter, instead of having to rear from the very foundation the edifice of its own knowledge.

What has been just said in regard to the form of our globe may, with even more propriety, be asserted of its movement of rotation. We shall admit, without discussion, that among the Indians, the Chinese, the Chaldeans, there might possibly be a few who recognized and maintained the reality of this movement; that the same might be true of ancient Egypt, and that certain Greek philosophers, especially of the school of Pythagoras, also taught at a later period the same truth. To explain the alternation of day and night two hypotheses were feasible, and there was nothing to forbid men of special talent adopting the more rational one; but was the merit of Copernicus, therefore, less in having reproduced the right idea about the middle of the sixteenth century? How many years must still elapse, how many angry and deplorable discussions ensue before the ideas of Copernicus became firmly established even among men of science and systematic cultivation. On the other hand, did the Greek philosophers, who admitted the rotation of the earth, build their doctrine on the difficulty of reconciling in any other manner the phenomena of the heavens, or

was it maintained rather in the spirit of the school, by which same spirit they might have been induced to support the direct contrary?* It is certain that Hipparchus, Ptolemy, Euclid, and Archimedes, eminent minds and founders of true astronomy, of geometry, and of mechanics, more versed certainly in observation and calculation than in the subtleties of metaphysics, denied the movement of the earth, and for many ages strengthened the opposite belief with their imposing authority. Hence this belief was the prevailing one when Copernicus appeared in the world to overthrow it, at the epoch of great geographical discoveries, as if the Creator had designed that after the form and distinct features of our planet were unveiled, its relations of analogy with the rest of the universe should also be disclosed.

Copernicus was not only a consummate methematician, a skilful observer, capable of deducing great results with rude and inefficient instruments, but he was likewise, as we are assured by his biographer, Czynski, a man of profound piety, full of faith in the wisdom of the Creator, and penetrated with the simplicity of his works. With these elements of character the astronomer of Thorn studied the movements of the celestial bodies, perceived their inextricable complication upon the principles then received, the infinity of occult agencies and of forces distinct in direction and intensity, which must concur in the operation to carry all the heavenly bodies around the earth without varying their relative distances, or altering in the minutest particular the harmony of the creation, and instead of confining himself to saying, with the sage King of Castile, "it is strange that this should be so," resolutely pronounces, "this cannot be so."

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* To show that we exaggerate nothing in thus expressing ourselves, we shall here retrace, with all possible brevity, the different opinions of the Greek philosophers on the form of the earth and its situation in space, making use for that purpose of the work by G. Lewis, entitled, *An Historical Survey of the Astronomy of the Ancients.*

Thales of Miletus, who flourished between 639 and 546 years before our era, likened the earth to a bark floating in a limitless ocean.

According to Anaximander, likewise of Miletus, and disciple of Thales, the earth was cylindrical, and occupied the centre of the created universe.

Anaximenes, a disciple of the former, assigned to the sun the form of a thin disk, and to the earth that of a trapezium sustained in the air, and the same opinion was entertained by Anaxagoras of Clazomene, likewise a philosopher of the Ionic school.

Xenophanes of Colophon, founder of the Eleatic school, supposed the earth to be illimitable and supported in the abyss on immovable foundations. Parmenides and Empedocles, dissenting from this opinion of Xenophanes, pronounced, perhaps before any one, the doctrine of the sphericity of the earth and of its isolation in space.

The cosmical opinions of the Pythagoreans, as stated by Philolaus, a disciple of the great master, were these: in the centre of the universe there exists a mass of fire, *the soul of the world*, around which revolve in a circle ten bodies in the following order: first and most distant, the heavens with the fixed stars; next the five planets; then the sun, the moon, the earth, and finally the *Antichthon*, a mysterious conception, which, indulgently interpreted, would seem to signify the terrestrial hemisphere opposite to that inhabited by ourselves. The basis of this system, one of the most judicious bequeathed us by antiquity, was purely mental, or the offspring of an invention governed by mystical abstractions and vague axioms respecting the virtues of numbers. To support it, instead of having recourse to the observation of natural phenomena, it was assumed, for instance, as a principle, that fire, being of a more exalted or worthy nature than earth, must by right occupy the place of greatest dignity, and that in any series of different bodies that place must correspond either with the centre or the extremes. From this reasoning the reader may form an estimate of the system of Philolaus, a system, however, which not all the Pythagoreans received without restriction and modification, for while some, as Hicetas, Heraclides and Epiphantus, attributed to the earth a movement of rotation from west to east, others, and among them perhaps Pythagoras himself, whose original ideas have not been transmitted to us, thought the earth immovable in the midst of the universe.

Leucippus and Democritus, both of the Atomic sect, maintained, towards the middle of the fifth century, like the Ionic philosophers, that the earth was a plane disk immovable in space and supported by the air.

It was in the early half of the fourth century before Christ that astronomy, based on the observation of the celestial phenomena, began to flourish among the Greeks. At that time

The great truth announced by Copernicus, the basis of existing astronomy, encountered at the time more opponents than partisans; nor was it possible that, in defect of good instruments and delicate observations, he could corroborate by incontestible facts the surprising revelations of his intellect; he could but consign to after ages the confirmation of his theory. In vain did Tycho Brahe, contrary to what might have been expected from his profound knowledge of celestial phenomena, impugn in the name of science, and that so late as the close of the sixteenth century, the astronomical system of Copernicus; in vain, at the commencement of the seventeenth, was it sought, in the name of more sacred but ill understood interests, to convert into a stumbling block the public belief in the movement of the earth: the truth wrought its own way, and from Galileo onward, for every adversary there were hundreds who sustained it. At present there is no longer any discussion about it; he who controverts it is regarded as irrational, and meets in universal indifference the reproof of his stolid incredulity.

II.

If the knowledge, whether certain or presumptive, of the ancient philosophers and mathematicians respecting the roundness and rotation of the earth, cannot be considered as the origin or basis of the ideas at present received on both those points, but merely as a remote antecedent completely forgotten at the revival of the discussion in modern times, the same thing nearly may be predicated of the researches undertaken to find the value of the radius of the earth's circumference. The analogy, it is true, is not entirely exact, for in these latter researches two things are to be distinguished: the method or principle on which they are founded, and the results finally obtained. The first as devised, two or three centuries before our era, by Eratosthenes and Posidonius, both of the school of Alexandria, is the same with that employed in our own time, as is shown in our *Annual* for 1862; the results of the method, whether from the imperfec-

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lived Eudoxus of Gnidus, a disciple of Plato, usually resident at Cizycum at the entrance of the Euxine, and one of the most distinguished among the learned of his time in the field, both of theory and practice. To explain the appearances of the heavens, on the hypothesis of the repose of the earth, Eudoxus conceived the first idea of crystalline spheres with axes in different directions, and also with different movements. New facts having been discovered, Calippus, a disciple of Eudoxus, in place of 26, admitted 33 spheres, a number which Aristotle found it necessary to raise to 55. These spheres, supposed at pleasure and symbolical of as many insoluble difficulties in the cosmical system followed by these savants, became established principles in the minds of the philosophers who had imagined them, as well as in those of their disciples, and consequently obtained unquestioned currency in the world.

Aristotle, taking up anew and analyzing the ideas of his predecessors, and rejecting almost all of them, a proof of their fundamental impracticability, admitted, however: 1st. That the earth is spherical, because such is the apparent form of all the firmamental bodies; such also the form which a body, as a drop of water for instance, assumes when left to the free gravitation of its particles; and such the form of the earth's shadow in eclipses of the moon. 2d. That the dimensions of the earth cannot be extended in an indefinite plane, seeing that with every change of place there is a change also in the aspect and number of the visible stars; and 3dly, That it cannot be movable in space, since its hypothetical mobility meets with no reflection in the constant position of other bodies of the universe. The system of Aristotle, based on the observations and conjectures of Eudoxus and his disciples, was that adopted by Euclid, Archimedes, Hipparchus and Ptolemy.

A generation after Euclid, who entitled one of his theorems "The earth the centre of the universe," and while the opinions of Aristotle and his followers were in the highest favor, there was a formidable protest against them advanced by Aristarchus of Samos, who flourished in the earlier half of the third century before our era, and who was one of the most distinguished luminaries of his age. Aristarchus exploded all the spheres of Eudoxus and Aristotle, set the earth again at liberty, assigned to the sun and stars their true position, and laid, in a word, the basis of the Copernican system; but in opposition to Aristarchus appeared Archimedes, on behalf of science, and Cleanthes, chief of the stoic sect, in defence of the faith and religious prepossessions of the age, and the happy conception of the sage of Samos remained sunk in oblivion, or passed into the category of dreams, until, in the process of time, it revived with new vitality and brighter evidence in the mind of the recluse of Thorn.

tion of instruments, the want of precision on the part of observers, or from having reached us in obscure expressions or in units vaguely understood, have been of no service to modern geometers. Five of these final results are cited by Bailly in his History of Ancient Astronomy, and these, doubtless, are not all that might have been cited; it is sufficient to compare them with one another, to perceive how little guarantee of exactness either of them affords *a priori*, or without subsequent corroboration. According to Aristotle, the opinions received in his time assigned 400,000 stadia as the circumference of the earth; Ptolemy adopted 180,000; Eratosthenes and Posidonius respectively 250,000 and 240,000; Cleomedes 300,000. The learned historian above mentioned explains these enormous discrepancies, which could not have resulted from the ignorance or dullness of the observers, in a sufficiently natural manner, by assigning a different value in each case to the stadium; assuming, as the state of knowledge at his time respecting ancient measures seemed to indicate, four kinds of stadia, approximately of 100, 136, 170 and 230, metres each, he arrives at the conclusion that these results, so discordant in appearance, are in the main identical, and not remote from those obtained by modern investigation. But Bailly himself, one of the most enthusiastic defenders of ancient science, agrees with us in thinking that the geodesic labors of the astronomers but little anterior to his own epoch, as well as those of his cotemporaries, were conducted in complete independence of the investigations of remote ages and without reference to a coincidence of numbers. Leaving to himself, therefore, the responsibility of his ideas, which we shall neither attempt to defend nor contravene, and judging this to be no occasion for reporting the earnest arguments adduced by highly respectable authors both for and against his views, let us concede, not to antiquity in general, but to a part of its philosophers, a knowledge, however loosely approximate, of the dimensions of the earth; and with this concession, let us pass to an exposition of the geodesic labors of times nearer our own and of more authentic character, though not all marked by an undoubted stamp of exactness.*

Towards the year 830 of our era, the Arabian astronomers measured, by order of the wise Caliph Almamon, an arc of the meridian in the plain of Sindgiar, near the coasts of the Red Sea; but the result of this operation made but little approach to the truth, or was either confusedly expressed at first or has been corrupted in the transmission.

In the year 1490, as Martin de Navarette relates in his compendious History of the Spanish Marine, our learned countryman Antonio de Nebrija, determined by various measurements and observations the quantity of a terrestrial degree, and obtained a number more near the truth than those before deduced. Subsequently, Glareans, in Switzerland, and Oroncio Fineo, in France, undertook and accomplished a labor analogous to that of Nebrija; and the same thing, with even better success, was effected by the French physician Fernel, who founded his estimate on the number of revolutions made by the wheel of a carriage in its transit from Paris to Amiens, cities situated under nearly the same meridian.†

In 1617, the Dutch astronomer Schnell revived the method of Eratosthenes, and applied it, with better means and more accuracy than had yet been observed,

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*The reader who may desire to know the slight or deficient foundations on which rest the conjectures of the authors who maintain the profound astronomical science of the ancients may consult the treatise of Sr. Vasquez Queipo, entitled, *Essay on the Metric and Monetary Systems of Ancient Nations*, tome 1, pp. 65, 66, and note 10, corresponding thereto.

† Upon the points here treated of, and other analogous ones not less deserving to be known, the reader will find critical notices of great value in the discourse relative to the progress of geodesy, read by Sr. Saavedra Meneses, at the beginning of the present year, on his reception into the Academy of Sciences.

to the measurement of the arc of 1° 11′ 30″ comprised between Alkmaar and Bergen-op-Zoom; but the result of his undertaking, calculated and discussed by Muschembroek, did not see the light until a later period, when others had been obtained of the same kind with higher pretensions to certainty.

In like manner with Schnell, Norwood determined, in 1635, the difference of latitude between York and London, equal to 2° 28′, by the difference of the altitude of the sun between their respective horizons at the period of the solstices; and afterwards measuring the distance between those cities, he arrived at a valuation, too great however, of the length of a degree of the meridian; making it 57,442 toises, or about 111,955 metres.

For its novelty, if nothing else, there should be mentioned in connexion with the preceding attempts the method proposed by Maurolico, at that epoch of measurements, to determine the terrestrial radius. Assuming the unquestionable fact that the extension or breadth of land which, seen from the sea-shore, or in the interior of a nearly level country, depends at once on the height at which the spectator is stationed, and on the curvature or radius of the earth, Maurolico thought that by measuring the height of a mountain near the sea and the route traced without change of direction by a bark until it disappears below the horizon, the value of the radius sought might be deduced, without reference to any astronomical observation. This process, put in practice at a later period, with some variations, and only by way of trial, has led to a result greater than might have been expected, being complicated with some causes of error and uncertainty; we do not know that the solution of the problem was ever attempted in the lifetime of its author.

Another idea, ingenious like all proceeding from the same source, occurred to Kepler, and Riccioli undertook to realize it, although in practice, from the imperfection of instruments among other considerations, it could not but lead to a result very distant from the true one. The idea consists in measuring upon any given surface of ground the greatest lineal distance possible, and then calculating the angles of the two respective verticals with the common line of vision comprised between the extremes of the base. It requires but a slight notion of geometry to comprehend how delicate was the operation which Riccioli took charge of, and how little reliance could be placed on results deduced from such a process.

These different estimates—for they merit no other name—towards ascertaining the magnitude of the earth, were but the prelude to other more exact processes, and show the necessity that was felt, but 200 years ago, of obtaining a precise knowledge of the dimensions of our planet, as well as the oblivion into which the labors of antiquity had fallen or the small importance attached to them. In proof of this, let us remember that at the end of the fifteenth century and beginning of the next, Columbus shaped his course towards the unknown shores of India and Magallanes traced his adventurous progress across the Pacific, upon the delusive supposition that the earth was of much less size than it really is; and that, in the midst of the seventeenth century, Newton himself, in whom the highest genius was not at variance with extensive erudition and a sound judgment, found himself under the necessity of suspending his researches respecting the reciprocally attractive action of the earth and the moon, in consequence of the want of an approximate valuation for the radius of our globe. So pressing did the necessity referred to appear, that when the Academy of Sciences of Paris was instituted, in 1666, one of its first acts was to commit to Picard, a distinguished member of that learned assembly, the measurement of a new arc of the meridian; a work which this astronomer completed before the end of 1670, by the method adopted by Eratosthenes and Posidonius, as well as by Schnell and Norwood, but which was executed with so much accuracy in the details as to form an epoch in the annals of astronomy and geodesy.

Between Villejuif and Juvisi, Picard measured a base of 5.663 toises, and by means of five triangles, resting on that line, deduced a distance from Mareil to Malvoisin equal, in the units just cited, to 32897. This line, much greater than the first, served him now for a base to connect Malvoisin with Sourdon, near Amiens, by a chain of triangles in the direction of the meridian. The arc comprised between the two last points was 1° 11′ 57″, and the distance deduced from the triangulation and projected on the meridian was 68.430 toises; whence there resulted for the value of a terrestrial degree the number 57064. Still later, Picard extended the operations to Amiens, and the degree then stood reduced to 57.057 toises; or, taking a middle term, to 57.060; a result which, assuming the sphericity of the globe, implied as the length of the earth's radius 3,269,300 units of the above name.*

Although in this memorable operation, on which we have dwelt somewhat, as being the first among those really worthy of confidence, Picard displayed great talent and activity, the result obtained was closely approximate to the truth only through a singular combination of errors; since, as appeared in the sequel, there was a very considerable one in the value of the first base, nor was that which existed in the quantity of the arc insignificant; two circumstances which, as they affected the result in opposite directions, were neutralized as regarded the operation itself, but were afterwards the source of much extraneous confusion and of long and warm discussions: a sad proof that imperfection, under some disguise or other, lurks in all the works of man; and that, without doing injustice to the memory or merits of the learned, we should never blindly surrender our belief to their authority.

The rotation of the earth being by this time a fact received without contradiction in the scientific world, of necessity soon drew with it its natural consequences: thus, the ideas of the less weight of bodies at the equator than in the neighborhood of the poles from the effect of the centrifugal force opposed to gravitation, and of the compression of the globe in the direction of the axis of movement, had begun to take root in all reflecting and unprejudiced minds, when an observation, in some degree unexpected, gave confirmation to this view of the question. The academician Richer, having been sent to Guiana, in 1672, for scientific purposes of different kinds, returned to his country the following year, and among other results of his expedition presented to the Academy an observation which, though incidentally made, proved to be the most important of all: the astronomical pendulum which, at Paris, gave an oscillation of one second, was found to move more slowly in Guiana, to the extent of making in a day 88 oscillations fewer than at the former point. This indicated an energy of gravitation at the equator inferior to that in high northern latitudes, the existence of the centrifugal force due to the rotary movement of

* The toise spoken of is that of France, containing 6.39459 feet, which dates from the time of Charlemagne, and is said to have originated with the Arabs. For many centuries the standard of this unit of measure was little known, and from time to time underwent modifications, the results of ignorance or carelessness more than of fraud, until in 1668 a new one was prepared and deposited in a secure place, in order to serve as a type for all of its kind; it was to this standard that Picard and the geometers who succeeded him referred their geodesic operations. A century afterwards the iron rule which was adopted for the standard of measurement of trigonometrical bases in Peru, also a toise in length, but better constructed than the toise of Picard's time and in a better state of preservation than the latter, was, at the suggestion of Condamine, declared to be the legal unit, at a temperature of 13° Reaumur, or 16° centigrade, that having been its medium temperature during the operations near the equator. The subsequent labors of Delambre and Mechain served to fix the length of the new lineal unit, or, in other words, of the metre, which, at the temperature of 0° is, in lines, 443,296, thus establishing between the metre and the toise the ratio of 1 to 1.94903631. This last number has been employed as factor in the remainder of this article, while it has been thought proper to convert the ancient units into the modern, or more usual of the decimal metric system.

the earth, and the great probability of the ellipticity of the globe. But as the ideas of attraction and of central and centrifugal forces had not as yet become familiarized, and as the phenomenon discovered by Richer might proceed from an unknown cause, the Academy suspended its judgment upon the consequences deducible from that phenomenon, until new and repeated observations should confirm or disprove them. The confirmation was not long deferred, for Halley, repeating four years after in St. Helena the same experiments which Richer had made, obtained an identical result, and the fact has subsequently been realized in all the regions of the earth as well as upon the high seas.

We may take this occasion to remark that in the study of nature there are problems whose solution, after resisting for ages all the forces of man, seems at some determinate epoch to become practicable in a hundred different ways; such a problem, undoubtedly, was that which now occupies us. In the sixteenth century Copernicus, Galileo, and whosoever thought as they did respecting the movement of the earth, were regarded with scorn or aversion; in the middle of the seventeenth, the roundness and rotation of the globe are admitted without difficulty; in 1670 Picard determines by a satisfactory process the value of the earth's radius; two years later, the observations of Richer show that the form of the globe differs sensibly from the spherical; about the same time, Cassini, by means of the telescope, perceives and measures the remarkable oblateness of Jupiter, thus supplying from analogy a weighty reason for admitting without other proof that of the earth; while Huyghens and Newton, preceding and directing, as it were, the methods of observation, deduce the same result by process of reasoning, establish the extreme limits within which its numerical expression must be comprised, and ascend to the cause from which it proceeds. Honorable epoch for the human intellect in which such capital discoveries rapidly succeed one another! With the songs of triumph, however, soon mingle the notes of discord, and for some years the problem of the figure of the earth remains stationary and proves to be beset with unexpected difficulties.

The Academy of Paris, stimulated by the prompt and apparently satisfactory termination of the measurement of the terrestrial degree by Picard, conceived the idea of prolonging the operations instituted by that savant from one extremity of France to the other, or, more precisely, from Amiens to Perpignan; a bold enterprise for that epoch, which the intelligent activity of Dominico Cassini realized in the latter part of the century. But when Cassini, the operations and calculations being concluded, compared with one another the values of the 7° of the meridian measured, he observed with surprise that their length continually diminished from south to north, as if the curvature of the earth increased towards the poles, or its radius diminished; or, in other terms, as if the compression of the globe corresponded to the equatorial region, contrary to all that was then conjectured or deduced from theory; a consequence which the same astronomer still arrived at after having prolonged the French meridian north from Amiens to Dunkirk, at the end of the year 1713. A conflict thus became unavoidable and imminent. On the one hand, the authority of Newton interposed itself; on the other, that, scarcely less weighty, of the French geometers, as well as the national pride of the latter; and as between the extremes in discussion there could be no possible compromise, the scientific world was divided into two parties; all that had been done or deduced to determine the true figure of the earth was brought before the tribunal of opinion, from the principle of the universal attraction of matter to the ability of the observers who had officiated in the measurement of the arc of the meridian. After much time had been lost in barren and heated discussions, the French Academy of Sciences, at the suggestion of Maupertuis and Bouguer, two of the most distinguished savants of their age, adopted the only feasible plan for setting the question

finally at rest. With this view, two delegations, composed chiefly of members of the Academy, and provided with the most delicate instruments for observing then known, were despatched, one towards the equator, the other to a high northern latitude, for the purpose of measuring one or more degrees of the meridian, from the comparison of which measurements, if effected with accuracy, might readily be deduced the direction of the terrestrial compression and its value, or the amount of divergence from a spherical form.

Maupertuis himself, assisted by Clairaut, Le Monnier, Camus, Outhier, and the Swedish astronomer Celsius, undertook the second of the operations referred to, proceeding to Lapland in 1736, as far as the 76° of latitude; and although it might have seemed that the rigors of the climate would present obstacles little less than insuperable, he had the good fortune to terminate his undertaking in scarcely more than two months. The triangulation extended from the mountain of Kittis at the north to the church of Tornea at the south; the base, of 7.407 toises, was measured upon the frozen river bearing the latter name, under conditions of exactness scarcely to be attained in any other climate; the quantity of the arc measured was 57′ 29″, and the resulting value of the degree of the meridian equal to 57.438 toises, or 378 more than the degree of Picard, as would be naturally the case on the supposition of the earth's being flattened towards the poles.

The other commission destined for the equator, and composed of Godin, La Condamine and Bouguer, had sailed a year earlier, or in 1735, and by order of the Spanish government was joined at Quito by D. Jorge Juan and D. Antonio de Ulloa, both worthy, from their zeal and intelligence, to co-operate with the French delegation. To recite the hardships to which these distinguished men were subjected during the eight years occupied in their prescribed task, the disappointments which they encountered, the deficiencies to be remedied, the precautions to be taken, and the sagacity and skill of which they made proof, would be beside our present purpose. Suffice it to say, that their measurement of the arc in Peru, notwithstanding the recent progress of practical astronomy, is still considered as a masterly operation in its kind.

The degree of Peru, of 56.753 toises, compared with that which Picard had measured in the north of France, pointed substantially to the same result with that already obtained by the collation of this last with the degree of Lapland; that is to say, to the polar compression of the terrestrial globe. To what, then, was it attributable that from the examination of the different degrees of the French meridian there resulted a diametrically opposite consequence to the above? From the fact before hinted at, that the first base measured by Picard labored under a considerable error, compensated, indeed, as regarded the final result by other errors of quite a distinct kind which were committed in the course of the operations, and which by a rare concurrence of circumstances operated in an opposite direction to the preceding. Without distrusting its exactness, Cassini also took that first line for the base of a triangulation much more extensive and important than that of Picard, and hence arose those incidental anomalies which involved the learned in so much confusion, until the illustrious La Caille divined from what source that incomprehensible difficulty emanated. The base in question having been rectified by successive admeasurements in 1740–1754, and the calculations corrected, the capital discrepancy, which till that date had interfered with the various geodesic results, disappeared.

In stating that the contradiction disappeared, we would only be understood to say that, after the epoch just referred to, there was a unanimous concurrence in the fact of the defective sphericity of the earth and the flattening of its poles; as regards the definite value of this, and the geometric figure to which our globe most nearly approaches, neither did such unanimity then, nor,

to our regret, does it still prevail; perhaps, indeed, the conditions of the problem forbid that it should ever do so.

From the values of the degree measured in Lapland and of the mean degree of France, there was deduced, as the expression of the terrestrial oblateness the fraction $\frac{1}{132}$; which means that, representing the equatorial radius by a length of 132 units of any kind, the polar radius would be 131 of the same. A comparison of the degree of Peru with the French gave for the value of this inequality the number $\frac{1}{304}$; that of the extreme degrees of Peru and Lapland $\frac{1}{210}$; while, according to Newton, theory assigned to this quantity the value $\frac{1}{230}$. Thus it was that, in a point so delicate and interesting, it still seemed difficult to know upon what to rely, notwithstanding the diligence and solicitude applied to the solution of the question in all its bearings.

It might have seemed that here were contradictions enough; but in proportion as other values of a degree of the meridian were determined, as by Boscovich between Rome and Rimini in 1754. by Beccaria in Piedmont in 1762, by Liesganig in Hungary and Austria in 1768, by Mason and Dixon in America, about the same period, and by La Caille near the Cape of Good Hope, new irregularities or anomalies were constantly encountered, incomprehensible upon any one principle, or inexplicable by the adoption of any regular and unique type, however complicated, as the figure of the earth. The confusion grew to such an extent that every one felt impelled to investigate its origin; and while some ascribed it to the physical conditions of the globe, admitting no assimilation of its form to any geometrical type, others imputed it to a defect of the instruments, others to the occasional oscitancy of the observers, and others again to errors of calculation. There was a little of all these. The calculations were revised and considerable errors detected, in the degree of Lapland among others; the observations were discussed, and were found not to be worthy of unrestricted confidence; the condition of the instruments was examined and was not found to be unimpeachable; in fine, since Bouguer first suspected it in his expedition to Peru until now, there have been encountered, in the local attractions of mountains and in the difference of thickness and of material in the crust of the earth, numerous causes of perturbation in the direction of the vertical—that is to say, of the first line of reference; which causes must necessarily exert an injurious influence on the final results of the observations. To whatever attributable, the fact remains, that till near the end of the last century the uncertainty respecting the value of the terrestrial flattening was complete. When we shall have finished the recital of geodesic operations conducted subsequently to those already mentioned, we shall see whether or not the same doubt exists at this advanced stage of the present century.

The idea of establishing a system of weights and measures whose fundamental unit, instead of being arbitrary, should present a simple relation to some important element of the same kind derived from the physical world, induced the republican government of France to order in 1792 a new measurement of the terrestrial globe. The operations instituted by Picard and continued by the Cassinis, Maraldi and La Caille, on account of the imperfection of the instruments employed and the errors and doubts involved, were deemed insufficient for the purpose; and Delambre and Mechain assumed the colossal task of renewing them from the beginning and completing them according to various criterions. Delambre exhibited his science and talent in the measurement of the French meridian from Dunkirk to Perpignan, and Mechain in the prolongation of this line through Catalonia to the coasts of Valencia. The labors of these two celebrated geometers having been concluded in 1799, the value of the earth's polar compression was, with the concurrence of an assemblage of savants of different countries, computed at $\frac{1}{334}$, and upon this computation the length of the *metre*, the base of the new system of weights and measures, was

taken as the ten millionth part of one quarter of the meridian just measured.* In 1803 Mechain passed anew into Spain with the intention of prolonging the arc of the meridian to the Balearic islands; but being placed in detention in the fortress of Montjuich, in consequence of the ill understanding then subsisting between his own government and ours, he took the occasion to rectify his former calculations and observations, and from the mortification which he experienced at observing certain discrepancies, fell into a state of dejection, and after having been previously set at liberty, died at Castellon de la Plana in the year 1805. During the two following years, Biot and Arago, assisted by the Spaniards Chaix and Rodriguez, not less worthy of participating in this work than Don Jorge Juan and Ulloa in that of Peru, carried the operation to the issue contemplated by the too scrupulous Mechain.

The British triangulation was initiated in 1784 under the direction of General Roy, with the twofold object of perfecting the geographical chart of the United Kingdom, and at the same time prolonging towards the north the measurement of a terrestrial meridian. After being suspended in 1788, these labors were resumed in 1793 under the supervision of W. Mudge, who extended the geodesic system to the extreme confines of Scotland, and deduced, as the value of the earth's compression, the fraction $\frac{1}{334}$, being identical with that obtained in France; yet the Spaniard Rodriguez soon after demonstrated that in the course of the British operations frequent and, to a certain extent, inexplicable anomalies were distinguishable.

After the preceding measurements the following are the principal ones in the order of their dates:

That of the arc of Lapland in 1801, undertaken with a view of verifying and extending the work of Maupertius.

That effected in India, in 1802, 1803, by Colonel Lambton, from which there resulted at first a flattening of $\frac{1}{305}$, which Rodriguez, in repeating the calculations, reduced to $\frac{1}{320}$. The same Lambton inaugurated another vast operation which, continued by Captain Everest, embraced an actual arc of more than 21°, from Cape Comorin to Kaliana, north of Delhi.

That of Piedmont, 1821 to 1823, conducted by the Italian astronomers Carlini and Plana.

That of the meridian of Dorpat, begun in 1817 and 1821 by Tenner and W. Struve, and which up to this time prolonged north and south from the frozen coast of Norway to the mouths of the Danube, comprises an arc of more than 25°.

Those of Hanover and Denmark, accomplished by Gauss and Schumacher, at the same date with the Piedmontese triangulation.

The Prussian, corresponding to the meridian of Köningsberg, which, under the superintendence of Bessel and Bäyer, exhibits a model in labors of this nature, and which it will be difficult for any future ones to excel.

Besides these important triangulations, still another deserves notice, which was effected by Maclear in the extreme south of Africa, with the object of

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* The calculations required to fix the length of the metre were executed by Swinden on the part of Holland, Tralles of Switzerland, Laplace and Legendre of France, and Ciscar of Spain. Delambre showed, not long afterwards, that, as well in the selection and analysis of the elements of the calculation as in the calculation itself, not all the circumspection desirable had been observed; a judgment which analogous works, effected in the course of the present century, have fully confirmed. The difference between the legal and the theoretic metre—a difference which will never be perfectly known—is, however, very small, and abates but little or not at all the merit of the decimal metric system, which possesses, in other respects, the most unquestionable advantages over other systems now in use. Still it is well to know that between the metre and the quarter of the meridian there does not exist the simple relation which was at first supposed, that unit having been reduced to a conventional type, as is also the case with all others of its kind.

On this subject may be consulted the *Tratado de Meteorologia Antiqua y Moderna, por M. Saigey.*

deciding whether the curvature of the two terrestrial hemispheres should be regarded as identical or distinct. At the end of the last century, as has been before intimated, La Caille had transported himself from France to the Cape of Good Hope, in the design of co-operating in the solution of various astronomical problems which in that remote country seemed to call for an intelligent observer, and having there executed the measurement of a small arc of the meridian, he obtained for the irregularity of the globe a much smaller value than any of the analogous ones deduced in Europe and America. How was this anomalous result to be explained? In one of two manners: either by attributing it to a real defect of symmetry in the form of the earth, or to an error, not easily to be avoided, in the operations of La Caille; but as the first was contradictory of the received theory and opposed to many facts well ascertained by other observers, and as the second was scarcely admissible in view of the recognized talent, industry and conscientiousness of the French savant, no one knew which alternative to adopt. Everest, on his return from India, inspected the locality where La Caille had operated, and at sight of the mountains which surround it concluded that the distinguished astronomer might easily have deceived himself, or neglected certain precautions without which no geodesic labor can really afford a sufficient guarantee of certainty. Maclear, with due regard to the indications of Everest, undertook in 1837 an operation analogous to that previously executed by La Caille, though on a larger scale and with better material resources; and the result now confirmed the previsions of the theory, or the identity of form of both terrestrial hemispheres.

Nor has it been only in a direction from north to south that astronomers and geometers have essayed to estimate the dimensions of the earth. When a comparison of the first results obtained in the proceedings directed to that object had revealed, not only the defective sphericity of our globe, but the irregularities or accidents which interrupt its presumed ellipticity, whether from one pole to the opposite, or even in passing from one meridian to another not far distant, the attempt was also made to measure one or more arcs of parallel. That by this means, as by the former, and still better by a combination of both, a knowledge of the form and volume of the earth might be obtained, is readily conceived; and when it is considered that this new operation is even more delicate and troublesome than the other, the reader will scarcely wonder that till a quite recent epoch the number of arcs of parallel measured bore no proportion to the arcs of meridian. The Franco-Spanish commission, charged with measuring an arc of the latter sort in Peru, proposed also to determine the value of a degree of parallel, which in those regions would have been a degree of the equator, but a difference of views as to the execution, added to the difficulties of the enterprise, led to a relinquishment of the project before it had begun to be carried into effect. At the same epoch, 1734 to 1740, the Cassinis, Maraldi and La Caille measured in France two arcs of parallel, one in the latitude of Paris, the other across Provence; and still later Lambton undertook in India a work of the same kind; but these first essays led to no definite result, and served only to show at once the utility of the undertaking and the difficulties which its adequate accomplishment would present. The measurement of a great arc of parallel, stretching from the neighborhood of Bordeaux to Padua, or from the ocean to the Adriatic, over an extent of 13° and at a latitude of 45° 43′, was commenced in 1811 under the direction of Colonel Brosseau, and continued in 1820 across upper Italy by Carlini, Plana and other astronomers and geometers of Italy, France and Switzerland. Besides this operation, which forms an epoch in the annals of geodesy, there must also be mentioned the measurement of another arc upon the parallel of Paris, from Brest to Strasburg, executed between 1818 and 1823, by the French functionaries Bonne and Henry; another completed from Greenwich to Valentia in the west of Ireland, by Professor Airy; and a third commenced in 1857 by W. Struve, in 52° of

latitude, designed to connect with the last, and thus embrace an arc of about 70° total length, from one extremity of Europe to the other.

It would be impossible, without overstepping the limits which discretion prescribes, to carry further this enumeration of geodesic labors already accomplished, or in course of execution, or projected for early realization. The earth would be seen to be covered with an immense net-work of triangles, whose meshes interlace more and more every day, so as to leave to truth thus earnestly sought less and less chance of finally evading detection. In this work of so many ages, where, more perhaps than in any other, man has displayed the talent and irresistible energy with which he is endowed, Spain is to-day taking an active and honorable part. The summits of our mountains, although constantly visited by distinguished military functionaries, resound not now with the echoes nor are seen clothed with the smoke of battle. They serve not as watch-towers of war, but as stations for geodesic signals, true symbols of peace and of culture.

But, as is opportunely asked, in order to dispel the doubt, by one of the most estimable intellects of our country,* To what end do so many measurements of the globe conduce? What practical result is expected from such laborious and persevering attempts? Of results to be appreciated by the material and tangible interests involved, perhaps none; but does science propose for its exclusive object the satisfaction of man's primary necessities? Hunger and thirst appeased, is there, indeed, nothing beyond? Wretched would be the science which would shut itself up within such narrow limits, which should restrict the soul to the care of its frail tenement, and seek in the secrets of nature no trace of its Creator, which should refuse to lift itself from the abject to the elevated, from the slough of earth to the etherial regions of infinity. And taking for granted that there is nothing fortuitous in the universe, and that the earth, instead of being spherical, is elliptical, or of a more complicated form, does not science fulfil its appropriate task when it investigates the true figure of this little globe of ours, not for the simple pleasure of knowing it, but with the further purpose of discussing the reason of that form, its origin, the changes experienced, the perturbations by which it may have been affected, the influence it exerts or the function it fulfils in the admirable co-ordination of the created whole? If all this is not worth the trouble of investigation, to what other mystery of the physical world should man, in preference, consecrate his studies?

III.

Having mentioned the principal geodesic operations which have, at different times, been effected in different countries, to determine the form of the earth, it remains only to indicate the manner in which the partial results deduced from those operations have been combined, in order to obtain the final result, which is, at present, regarded as most approximate to the existing reality. Three distinct modes have been successively adopted for arriving at the proposed end.

As the result of inexact observations, and an incomplete theory, it was first assumed that the earth's figure was perfectly spherical. The labors of Picard, and of the French geometers, who immediately followed him, conclusively demonstrate the fallacy of this supposition; since, in contradiction of such an hypothesis, very different terrestrial radii were found to result from the several degrees of meridian measured, and within limits too wide to admit of the inference that these differences were, collectively, attributable to errors of observation, or mistakes in calculation.

The laws even then recognized, of the universal attraction of matter, the aspect of certain planets, such as Jupiter, which exhibit a flattening towards

* Sr. Vasquez Queipo: Disquisition on the Discourse of Señor Saavedra Meneses, before cited.

their poles, or the extremities of the axis of rotation, and an induction founded on well-proved facts, showing that the terraqueous globe existed, at some very remote period, in a state of perfect fluidity, furnished sufficient grounds for concluding that the earth, instead of being spherical, would naturally present an elliptical figure, or one slightly depressed in the direction of the polar axis. This being conceived, it remained simply to deduce from geodesical operations the value of the depression, or, what amounts to the same thing, the relation of the two axes of the generating ellipsoid, as well as the definite dimensions of those axes, for all which it had, in strictness, sufficed to measure two small arcs of meridian in widely separated latitudes—one, for instance, near the equator, the other in some inhabitable region nearest to either pole; nor, on the above supposition, would it have been of consequence whether those arcs corresponded to the same or to different meridians, while any intermediate arc, which might be measured, would serve for the verification of the former, as well as of the law of ellipticity, assumed as a point of departure. When the results of the scientific expeditions to Peru and Lapland were known, and were compared in the proposed view with those obtained in France, and for the first time the values of the oblateness of the earth and of the equatorial and polar axes were deduced, it was observed, not without surprise, that between the final deductions drawn, with the aid of so much experience, and with the theoretical ideas generated by those laborious investigations, there did not exist all the conformity which had been hoped for. The discordance, however, was at once attributed, not so much to the defect of the theory, as to the errors, to a certain extent inevitable, which had been committed in the course of the operations, or to local irregularities in the surface of the earth; but, as time advanced, and instruments were improved, while the obstacles already overcome served as useful indications to succeeding observers, the conviction was acquired, either that the form of the earth was not so simple and regular as was at first supposed, or, more probably, that the heterogeneity of its mass, and the inequality of the thickness of its crust, acting as disturbing causes, embarrassed the labors of geodesy, and opposed their indefinite advancement. Certain it is, at any rate, that at the close of the last century, as has been already intimated, great indecision prevailed as to the real value of the earth's ellipticity, and that, but for the resort to an ingenious mode of eluding the difficulty, the same doubt would have prevailed on this point to the present day. A single citation will prove the truth of what has been just said. The Russian general, Schubert, a distinguished mathematician and astronomer, collated, in a memoir published at St. Petersburg in 1859,* the elements of the eight principal arcs of meridian yet known, being the Russian arc, measured by Hansteen, Selander, Struve, and Tenner; the Prussian, the English, and the French arcs; the arc measured in Pennsylvania by Mason and Dixon; that in Peru, by the Franco-Spanish commission; that in India, by Lambton and Everest; and that measured at the Cape of Good Hope by Maclear. By combining these eight arcs, two by two, in all possible manners, the Russian savant deduced, for the elements of the terrestrial ellipsoid, twenty-eight different results, between limits much wider, doubtless, than the reader would imagine. Limiting ourselves, for example, to the polar compression, the twenty-eight valuations just cited group themselves in this manner: Three are higher than the fraction $\frac{1}{200}$; four are higher than $\frac{1}{250}$, and lower than the preceding fraction; nine are comprised between the last and $\frac{1}{300}$; seven between that and $\frac{1}{350}$; three between $\frac{1}{350}$ and $\frac{1}{550}$; and two, finally, being those corresponding to the combinations of the Russian with the Prussian arc, and of the arc of the Cape with that of Pennsylvania, are lower than the fraction $\frac{1}{1200}$. Supposing even that there were good and sufficient reasons for subtracting from the extreme values, it will still be

* *Essai d'une Determination de la Veritable Figure de la Terre.*

seen from this slight analysis of Schubert's work that there is a wide field for the exercise of doubt.

If it be conceded that the spherical figure of the earth is not admissible, and the elliptical appears as little accordant with the most probable results of observation, what other geometrical type will represent, better than these two, or more approximately than the second, the general form of our globe? None, in fact; for neither the more complicated figure, which Bouguer imagined, nor the idea of separating the axis of symmetry from the polar axis, suggested by Klügel, conceptions, both of them, which the theory of the attraction and primeval fluidity of the earth excludes, are found to be exempt from the grave inconveniences which oppose themselves to the adoption of the second supposition. Of this truth Schubert himself supplies us with a good proof. In his memoir above cited, after analyzing the divergences, with reference to the form of the earth, according to the elements from which that form is deduced, and investigating the causes from which so great a discordance might proceed, he concludes by maintaining that the earth resembles, not so much an ellipsoid of revolution, as an ellipsoid of three axes, or, what is the same thing, that the meridians are to be regarded as unequal ellipses, and the equator and parallels as also ellipses, and not as circles, as had, till that date, been believed. But the same astronomer, who seems so well persuaded of this consequence from his first investigations in April, 1859, affirms, in January, 1861,* that, setting aside the arc of India, he does not find, in the rest of the geodesic operations, any grounds for doubting that the terrestrial globe is an ellipsoid of revolution, compressed in the direction of the poles. What does this change of opinion, this vacillation, in a man of Schubert's merit prove, if not that this last figure represents that of the earth, as far as a geometrical abstraction can represent the forms. full of life and harmonious adaptability, of natural objects?

But, admitting the elliptical form, it still remains to determine its constitutive elements, and its dimensions; and, with this view, what is the combination of arcs of meridian which should be preferred to the rest, whether for the precision with which those arcs have been measured, the merit of the geometers to whom the operations were intrusted, or the favorable circumstances of time and territory in which they were executed? No single combination whatever: First. Because all that astronomers of merited reputation and conscientiousness profess to have done should be considered to be well done, or, at least, to be comparable with what other astronomers, endowed with the same qualities, are capable of realizing, under the penalty of introducing into the science a principle of endless confusion. Secondly. Because the differences which occur in the elements of the terrestrial ellipsoid, taken by separate combinations of arcs of meridian, indicate, not so much a defect in the operations, or a fault in the observers, as a real irregularity in the form of the earth, or the existence of disturbing causes, such as the local attraction of mountains, and even those, scarcely avoidable in practice, which proceed from the unequal density and thickness of a plane surface. And thirdly. Because if, in all strictness, the form which we seek does not coincide with the preconceived figure, the interests of truth will always vindicate their claim to recognition, if not by an apparent simplicity, at any rate by other more fertile qualities than pertain to any theory, however simple and seductive. In order, then, to deduce the geometrical figure of the earth the proper course would seem to be to take into view all the partial measurements which have been made, or such, at least, as are distinguished by some notable circumstance, as the place to which they correspond, the extent they embrace, or the accuracy which has marked their execution, rejecting, of course, all which manifest carelessness on the part of the observers, or defect in the instruments which they have been obliged to employ; and, assuming that the ellipsoid of revolution is,

*_Astronomische Nachrichten_, No. 1303.

in theory, and to a certain point also by experiment, the hypothetical figure most conformable to reality, the final problem, one of pure mathematical analysis, and not certainly exempt from difficulties, will consist in finding, by a collation of the values of the several arcs of meridian and parallel already measured, or hereafter to be measured, the curvature and dimensions of the ellipsoid of the above species, which, without exactly satisfying one or two geodesical operations, represents the results of all with the closest possible approximation. In this difficult labor the Germans, Walbeck and Schmidt, by combining, respectively, six and seven degrees of meridian, Bessel ten, Airy fourteen of meridian and four of parallel, and, finally, the Englishman, Colonel H. James, eight arcs of the former kind, which afforded the greatest assurance of exactness, arrived independently at results closely coinciding with one another, each of which might serve, in the absence of the rest, for a definite solution of the problem with which we are occupied. In the first of the two following tables, taken, though not entire, nor in the form here presented, from the Annual (Jahrbuch) of the Observatory of Berlin, for 1852, are shown the principal values given by the above mathematicians, together with the elements of the ellipsoid, which served for the establishment of the decimal metric system, in the calculation of which, as was before said, only the results obtained in Peru, France, and Lapland were taken into account, and that, too, before these were competently known. In the second table are presented other values, relative likewise to the form and volume of the earth, deduced from the fundamental elements of the globe, calculated by Bessel, and not less worthy of attention than those contained in the former table. The initials employed in both tables signify as follows:

In the first, R and r, the equatorial and polar radii; D, their difference; C, the polar compression of the globe, or the difference of the radii referred to the greater; e^2, the square of the eccentricity of any meridian ellipse, or, say, the difference of the squares of the two principal radii, referred to the square of the equatorial radius; Q and q, the values of the equatorial and meridian quadrants; and D and d, the values of a single degree of the equator and of a mean degree of meridian, computed in metres like all the preceding which do not express abstract relations.

In the second table the sign φ marks the latitude or distance from the equator of the place or point to which the numbers on the right refer; M expresses the value of an arc of meridian of a single degree, comprised between the first and the corresponding latitudes of the margin; P, that of a degree of parallel; R, the terrestrial radius or distance of the surface from the centre of the earth variable with the latitude; and A, the area in square kilometres, comprised between two meridians separated by a degree of the equator and two parallels, between which intervenes a degree of meridian for different latitudes.

Elements of the terrestrial ellipsoid.

TABLE 1.

	(1799.)	Walbeck, (1819.)	Schmidt, (1829.)	Bessel, (1841.)	Airy, (1849.)	James, (1858.)
R	6375739	6376895	6376959	6377397	6377480	6378283
r	6356650	6355832	6355522	6356079	6356175	6356686
D	19089	21063	21437	21318	21305	21597
C	$^{1}334.00$	$^{1}302.78$	$^{1}297.48$	$^{1}299.15$	$^{1}299.33$	$^{1}294.26$
e^2	0.005979	0.006595	0.006712	0.006674	0.006671	0.006785
Q	10014988	10016803	10016904	10017592	10017722	10018983
q	10000000	10000268	10000074	10000856	10000996	10001966
D	111277.6	111297.8	111298.9	111306.6	111308.0	111322.0
d	111111.1	111114.1	111111.9	111120.6	111122.2	111133.0

TABLE 2.

ϕ.	M.	P.	R.	A.
0	"	111307	6377397	"
1	110564	290	7391	12306
2	565	239	7372	302
3	566	155	7340	295
4	568	037	7296	284
5	571	110886	7239	269
6	574	701	7168	251
7	578	482	7085	229
8	583	230	6990	204
9	588	109945	6882	175
10	594	627	6761	142
11	600	275	6628	106
12	608	108890	6483	066
13	616	472	6327	022
14	624	021	6160	11975
15	633	107538	5981	924
16	643	022	5790	870
17	653	106474	5588	812
18	664	105893	5376	751
19	675	280	5154	686
20	687	104635	4922	618
21	700	103958	4680	546
22	713	250	4428	471
23	726	102510	4166	392
24	740	1739	3895	310
25	754	100938	3616	224
26	769	106	3228	135
27	784	99246	3033	043
28	800	8350	2730	10947
29	816	7427	2419	848
30	833	6475	2102	746
31	110849	95493	6371778	10640
32	867	4482	1447	531
33	884	3442	1111	419
34	902	2373	0770	304
35	920	1277	0424	186
36	938	0153	0073	064
37	956	89001	6369718	9939
38	975	7822	9360	811
39	994	6616	8998	681
40	111013	5384	8633	548
41	032	4125	8266	411
42	051	2841	7897	271
43	071	1531	7526	129
44	090	0196	7154	8983
45	110	78837	6783	835
46	129	7454	6412	684
47	149	6047	6040	531
48	168	4616	5669	375
49	188	3163	5299	216
50	207	1687	4931	054

TABLE 2—Continued.

φ.	M.	P.	R.	A.
51	226	0189	4565	7890
52	245	68670	4202	724
53	264	7130	3842	555
54	283	5569	3486	384
55	301	3987	3134	210
56	320	2386	2786	034
57	338	0766	2442	6856
58	356	59127	2103	676
59	373	7470	1769	493
60	391	5794	1442	308
61	408	54101	1122	121
62	424	2392	0809	5933
63	440	0667	0503	743
64	456	48926	0204	550
65	472	7170	6359913	356
66	111487	45399	6359631	5160
67	501	3614	9358	4963
68	515	1816	9094	762
69	529	0005	8838	563
70	542	38182	8592	361
71	555	6347	8355	157
72	567	4500	8129	3952
73	578	2643	7914	746
74	589	0775	7711	539
75	599	28898	7518	330
76	609	7012	7337	120
77	619	5118	7167	2909
78	627	3216	7009	697
79	635	1307	6863	485
80	642	19391	6728	272
81	649	17469	6606	057
82	655	15542	6496	1842
83	661	13610	6399	1628
84	666	11673	6315	1412
85	670	9733	6244	1195
86	673	7790	6185	979
87	676	5845	6138	762
88	678	3898	6106	544
89	679	1950	6087	327
90	680	,,	6079	109

Total area .... 509,950,715 square kilometres.
Volume .... 1,082,841,311,330 cubic kilometres.

From the examination of the first of the above tables it results that, if we adhere to the geodesic operations alone, the number $\frac{1}{300}$ expresses by how much the terrestrial globe differs from the spherical form, and the numbers 6,377 and 6,358 (kilometers) give the dimensions of the greater and less or equatorial and polar radii, an approximation which might, in all probability, be qualified with a higher degree of exactness by adopting either the fundamental elements given by Bessel, the astronomer of the most enviable reputation of the current century, or those deduced somewhat later, and, of course, from more copious data by the distinguished director of the Observatory of Greenwich, Professor Airy. The

exactness, or, at least, close approximation of the number $\frac{1}{300}$, is found, moreover, to be confirmed by another class of considerations extraneous, in a certain degree, to geodesy, and very indirectly related to those which served the two celebrated astronomers last mentioned as a basis and guide in their valuable labors of combination and analysis.

It was remarked at the close of the second part of the present article that nothing in nature is fortuitous; and it might well have been added that not only is nothing fortuitous, but there is nothing without a reason for its being as it is, nothing susceptible of being essentially modified without communicating an impression to other organic parts of the complicated mechanism of the universe. The movement by which the moon is carried around the earth does not depend exclusively on the intervening distance or the respective masses of the two bodies, but on the distribution of their masses in concentric groups or on the figure of both globes. If the earth were spherical, the movement of its satellite would not be that which is always observed; nor if the discrepancy from that simple form had been represented by a fraction differing from $\frac{1}{300}$ would this fact have failed to disclose itself in a degree more or less sensible in some of the accidents which characterize the lunar movement: theory, based upon the laws of universal attraction, laws announced by Newton and so sagaciously developed by Laplace, indicated the orbit which the moon was destined to describe on the hypothesis of the polar depression of our globe being less by $\frac{1}{300}$ than the equatorial radius, and observation promptly confirmed all the conclusions to which the theory had pointed. Few astronomical discoveries reflect more honor on the human intellect than the valuation of the earth's ellipticity based upon the principles which have been just cursorily mentioned.

But it is not necessary to withdraw our eyes from the globe we inhabit to discover other means, besides those which are strictly geodesical, not only of demonstrating the ellipticity of its form, but of verifying the limits within which the eccentricity of that new figure is comprised. Our readers will doubtless readily infer that the process alluded to consists in the use of the pendulum, whose oscillations are more or less rapid in different parts of the earth, by reason of its form being sensibly and essentially different from the spherical. When Laplace announced the relation existing between the movement of the moon and the oblateness of the earth, Clairault, in a special treatise on the subject, had already stated the law of interdependence by which the continuous depression of the globe from the equator to the poles is associated with the variations of gravitation or of the weight of bodies, and consequently with the oscillatory movement of a pendulum on the surface of that globe. By both geometers the task of verifying the truth of their theories was bequeathed to after experiment, and in both cases the previsions of mathematical analysis and the results of observations long and carefully repeated have been found to be perfectly accordant.

In the long period which elapsed from the date when the French academician Richer first noticed the retardation of the pendulum in the equatorial zone, to that when the Spanish admiral, Malespina, undertook his justly celebrated voyage of scientific exploration in 1789, the experiments made with the pendulum were numerous and interesting, in so far as they were directed to the demonstration of the ellipticity of the earth and the accidental irregularities which distinguish it; but those undertaken with a view to determine the value of that ellipticity have been neither so many nor were they so early as the former. In 1826 Bessel showed the inaccuracy or want of care in the process till then followed for deducing from the oscillations or length of a compound pendulum, moving in air and at a variable temperature, the corresponding elements of a simple pendulum, oscillating in a vacuum and in a thermal state of absolute invariability: and, even much later, Humboldt thought that experiments with the pendulum, comparable in delicacy and precision with the

multitude of other, the most common, astronomical or geodesical operations, would scarcely amount in number to sixty. So scanty a result should, in our opinion, be attributed to two quite distinct causes. In the comparative experiments made with the pendulum, there is sought, in the first place, a difference of length or of numbers so small that the least inadvertence in the operation, or a disturbing cause unworthy elsewhere of consideration, will materially influence the result and impair its exactness. And moreover, even when the observations are conducted throughout with all the requisite accuracy—a thing, we repeat, of great difficulty—still the theoretic principle of their combination for deducing the terrestrial ellipticity supposes that the density of our globe, though variable according to an arbitrary law from the surface to the centre, continues identically the same in each layer concentric with the superficial one; an hypothesis which departs in some degree from the reality of nature, and which on that account cannot lead to results of absolute certainty. After these considerations, it will not be a matter of surprise that the values of the terrestrial ellipticity, deduced from experiments made in the present era by Borda first, and afterwards by Biot, chiefly at different points of the French meridian, by Kater in England, by the navigators Freycinet, Duperry, Sabine, Foster and others, under very different and distant latitudes, should sensibly vary from one another, and likewise to some extent the final number, deduced from the examination of all of them, when compared with that which results from the sum of the principal geodesic labors. But to what at most does the difference amount? From the experiments made with the pendulum, there results as the value of the earth's polar compression the number $\frac{1}{290}$, somewhat greater than the fraction $\frac{1}{300}$ and less than $\frac{1}{270}$; the difference of these two extreme fractions is equal to $\frac{1}{2700}$; so that the difference of the results obtained by help of the pendulum and by the ordinary processes of geodesy will be found to be represented by a number still less than the last. Admitting, then, that the value of the equatorial radius is in metres 6,377,397, there would remain in the length of the polar radius an uncertainty of 2.362; and this, it must not be forgotten, on the supposition, really more unfavorable than is warranted, that the doubt respecting the polar compression of the earth would present to us as equally uncertain the two fractions $\frac{1}{300}$ and $\frac{1}{270}$. But the relation of the number 2.362 to the value of the equatorial or the polar radius is lower than that of 1 to 1000: thus in the appreciation of a quantity composed of a thousand equal parts, it would be at last doubted whether we had counted one part more or less than was proper! Instead of being surprised at the existence of such an uncertainty as this, it might well cause astonishment, as Prof. Airy has remarked in reference to this subject, that man should have arrived at a knowledge so precise in a matter so difficult and obscure; while there is still room for confidence that further advances are in his power, and adequate encouragement to persist in the pursuit of the truth.

That this confidence and encouragement exist is shown by a simple reference to the projects of new geodesical operations and experiments, suggested by some of the most celebrated of cotemporary astronomers. In 1857, for example, Biot proposed to the Academy of Sciences of Paris that a new determination should be undertaken, by methods and with instruments more delicate than were before known, of the whole extent of the arc of Peru as well as of the various arcs of parallel measured in Europe; that experiments with the pendulum should be multiplied in those localities where considerable anomalies have been noted in the direction of the vertical, or where their existence is suspected, with a view to ascertain their cause or causes; in a word, that no means should be spared of discovering all the accidents of form and density which distinguish the terraqueous globe from the theoretical ellipsoid defined by Bessel and the mathematicians who, with a degree of precision difficult to surpass, have either preceded or followed him in this enterprise. The ideas of Biot, deliberately considered and digested eventually into the colossal project of measuring a new

arc of meridian extending from Palermo to the parallel of Cristiania and Upsal, across seas and continents prodigiously diversified, and intermediate to the Russian arc in the east and that stretching from Formentera to the Shetland isles in the west of Europe, have been zealously seconded by the Prussian general Baeyer, the companion of Bessel in the geodesic operations of Koenigsberg, and distinguished alike for his knowledge and experience. In the memoir relative to this matter, which he published in Berlin in 1861, Baeyer does not ask the protection of goverments, nor invoke the learned of all countries to unite their efforts, for the purpose of ascertaining whether the polar compression of the earth is a hundred-thousandth part greater or less than it is believed to be; he holds, on the contrary, that the geometrical problem is resolved; but the physical and geological problem, closely associated with the real figure of the globe, he regards as scarcely yet defined. The idea of Baeyer, which Biot, as we have seen, also cherished, and which equally exercises the thoughts of other savants, would doubtless be realized, if the local influences which embarrass and complicate the geodesical operations, instead of being avoided as heretofore, were purposely sought for and measured; if, wherever practicable, the net-work of triangles were extended around and over the surtace of seas and of volcanic regions, and across the valleys and mountain-chains of more abnormal composition; if the instruments for measuring distances and angles were rendered comparable in some sort to the balance of the chemist and the goniometer of the mineralogist; in brief, if, after having defined the external figure of the earth, geodesy should penetrate, as it were with the eyes of induction, into the interior of the globe, in order to reveal to us the origin of that figure, the transformations it has experienced, and the stability, whether little or great, which it possesses for resisting the destructive assaults of time. Considered under this new aspect, the question presents an extraordinary interest, opens to view an indefinite and almost unexplored horizon, and affords one proof more of the close interconnexion which exists among all the natural sciences. Let the project of Baeyer or some analogous one be transferred to the field of practice, and the nineteenth century will have won yet another title to the consideration of the ages to come.

AERONAUTIC VOYAGES

PERFORMED

WITH A VIEW TO THE ADVANCEMENT OF SCIENCE.

Translated for the Smithsonian Institution from the works of Francis Arago, late secretary of the French Academy of Sciences, &c.

I.—THE INVENTION OF BALLOONS.

MAN, by reason of his weight, and the weakness of his muscular power, seemed doomed to creep on the surface of the earth, and to have been disqualified for studying the physical properties of the higher regions of our atmosphere, except through the toilsome ascent to mountain summits. But what difficulties are there over which genius, united with perseverance, will not eventually triumph? From the most remote times the idea of soaring into the air, far above all terrestrial objects, by means of machines which the imagination endowed with properties unfortunately of impossible attainment, has never ceased to occupy the human mind. Who has not heard of the attempts of Dedalus and Icarus, of the projects of Roger Bacon, and of Fathers Lara and Galen? But, until 1783, it had been granted to no one to realize the dream of so many ages. Joseph Michel Montgolfier, who was born in 1740, at Annonay, in the department of the Ardéche, and who died, a member of the Academy of Sciences, in 1810, had calculated that through the rarefaction, by means of heat, of the air contained in a paper balloon of a certain extent, an ascensional force might be given it sufficent for elevating men, animals, and any desired instruments. So much confidence had he in his theory that he did not hesitate to undertake, June 5, 1783, a public and formal exhibition before the deputies of the provincial estates of Vivarais, assembled at Annonay. Montgolfier has himself described, in the following terms, this first experiment, which forms an epoch in the history of the most important discoveries: "The aerostatic machine was constructed of canvas, lined with paper, and covered by a network of twine attached to the canvas. It was nearly of a spherical form, and of a circumference of 110 feet, ($35^{m}.73$;) a frame of wood, 16 feet square, steadied it on its base. Its capacity was about 22,000 cubic feet. It, therefore, displaced, supposing the mean weight of the air equal to $\frac{1}{800}$ of the weight of water, a mass of air equivalent to 1,980 pounds, (969 kilograms.)

"The weight of the gas (heated air) was nearly half that of the air, for it equalled 990 pounds, and the machine, with the frame, weighed 500 pounds. For the rupture of equilibrium there remained, therefore, 490 pounds, as was found conformable to the experiment. The different pieces of the balloon were fastened together simply by means of button-holes and buttons. Two men sufficed to lift and fill it with gas, but it required eight to retain it. When released, at a given signal, it mounted with an accelerated velocity, though less rapid towards the end of the ascension, to the height of 1,000 toises, (upwards of 6,000 feet.) A wind, scarcely perceptible at the surface of the earth, bore it to the distance of 1,200 toises from the place of departure. It remained ten minutes in the air. The loss of gas by the button-holes, needle punctures, and other imperfections, prevented any longer suspension. The wind was, at the time, southerly

with rain. The balloon descended so lightly that it broke neither the branches nor frames of the vineyard on which it finally rested."

The gas employed in this experiment was nothing but air dilated by heat, but its nature was not stated in the report of the ascension published in the journals. Without waiting for further indications, the artist Robert and the physicist Charles, by means of a national subscription, which was readily advanced, constructed, of lutestring coated with gum elastic, a balloon four meters (13.12 feet) in diameter, which they filled with hydrogen gas, procured by the action of diluted sulphuric acid on iron filings. This balloon ascended from the Champ de Mars, August 27, 1783, at five o'clock in the afternoon, in presence of an immense crowd, and heralded by salvos of cannon. It remained but three-quarters of an hour in the air, and fell at Gonesse, near Econen, five leagues distant from Paris. Thus was demonstrated the possibility of making balloons of varnished material, nearly impermeable by hydrogen, the lightest of known gases, and possessing great advantages over the heated air. Yet this means of obtaining very considerable ascensional force with balloons of limited dimensions was not immediately adopted, and sundry experiments were successively made with very large aerostats inflated with air heated by a fire of straw mixed with a little wool. It was with such a balloon, having an oval form, a height of 23 meters, a diameter of 15, and a capacity of 2,056 cubic meters, that Pilatre de Roziers and d'Arlandes made the first aerial voyage which man had ventured to undertake in balloons wholly detached and unconfined. Ascending from the Chateau de la Muette, November 21, 1783, they traversed a distance of two leagues at an elevation of about 1,000 meters, having, in their transit, hovered over Paris for 20 or 25 minutes. The 1st of December following, Charles and Robert ascended from the Tuilleries in a spherical balloon, made of lutestring coated with gum elastic, and having a diameter of only 8.50 meters, which was inflated with hydrogen. After a passage of about nine leagues the balloon touched the earth at Nesles, where Robert left the car, while Charles reascended and reached an elevation of about 2,000 meters, alighting finally two leagues further on, after having experienced a cold of -5°, or $+23$ Fah., when the thermometer indicated on the ground $+7^{\circ}$, $44\frac{3}{5}$ Fah. From this day dates the demonstration of the practical possibility of balloon voyages—voyages always adventurous, but which have become, at a later period, a pastime with persons of leisure. I shall not speak here of the attempts which have been made to derive advantage from aerostats in military expeditions, nor of the numerous contrivances to direct their course through the air, nor of the unfortunate experiment of uniting the action of fire with the employment of hydrogen, for which Pilatre de Roziers atoned with his life, nor of the substitution of illuminating gas for hydrogen, a substitution which renders these enterprises less costly, but which diminishes the ascensional force of apparatus of a determinate dimension. I must restrict myself to aeronautic voyages, performed with a view to the advancement of science.

We must refer to the old Academy of Sciences if we would find an account of the first voyages by which science was benefited through the employment of balloons, in which hydrogen gas was used as an agent. The expeditions of MM. Biot and Gay Lussac, made in 1804, were preceded by the ascensions of Robertson, Lhoest, and Sacharoff, which yielded some interesting results; but not until after nearly half a century were the remarkable voyages of MM. Barral and Bixio undertaken, followed shortly afterwards by those of Mr. John Welsh.

II.—RESEARCHES TO BE MADE IN AEROSTATIC ASCENSIONS.

Those who propose to undertake aerial voyages form, in general, no idea of the number of questions to be resolved, nor of the difficulties to be surmounted in order to furnish science with certain elements of discussion. The instruments requisite for investigating, as well the temperature as the hydrometric

state of the air, the phenomena of the magnetic needle, the proportions of polarized light contained in the light of the atmosphere, the diaphaneity, the color more or less blue of the different strata of air, &c., do not exist at all, or else require important modifications before being applied to the research of the laws by which the phenomena vary with the height, which is itself not determined with entire precision by barometrical observations. For half a century many learned bodies—the French Academy of Sciences, that of St. Petersburgh, the British Association for the advancement of science, the Academy of Dijon, &c.—have directed inquiry to the means of supplying the defect of which I speak, and of furnishing aeronauts with adequate instruments of investigation. But the problem has been by no means considered under all its aspects, and is very far from having received a complete solution; at all events, the suggestions which have been derived from the voyages of Biot and Gay Lussac, and especially from those of Barral and Bixio, should be taken into serious consideration by those whose zeal shall hereafter prompt them to encounter the perils of such enterprises, in the view, particularly, of reaching the most highly rarefied aerial regions, and traversing the atmosphere under its most variable conditions. The principal questions on which the attention of such explorers should be fixed are the following:

1. The law of the decrease of atmospheric temperature with the elevation.
2. Influence of the solar radiation in the different regions of the atmosphere, deduced from observations made upon thermometers whose bulbs are coated with very different absorbing substances.
3. Determination of the hygrometric state of the air in the several atmospheric strata, and comparison of the indications of the psychrometer with the dew-point at very low temperatures.
4. Analysis of the air from different heights.
5. Determination of the quantity of carbonic acid contained in the higher regions of the atmosphere.
6. Examination of the polarization of light by clouds.
7. Observation of different optical phenomena produced by the clouds.
8. Observation of the diaphaneity, and of the intensity of the blue color of different strata of air.
9. Observation of the declination and inclination of the magnetic needle, and of the intensity of magnetism.
10. Study of the electric state of different atmospheric strata.
11. Experiments on the transmission and reflection of sound in different strata of air in a serene state of the sky, and in a sky containing clouds.
12. Physiological observations on the effects produced by the rarefaction of the air, very low temperatures, extreme dryness, &c.

The instruments at the disposal of the voyagers should be the same as those which, by my own advice, and that of my illustrious colleague, M. Regnault, were carried by MM. Barral and Bixio in their expeditions, and which they would have continued to use had they been able to make other ascensions, to wit:

1. Two siphon barometers, graduated on glass, of which the aeronaut need observe only the upper meniscus, the position of the lower meniscus being given by a table constructed after direct observations made in the laboratory. Each of these barometers should be provided with a thermometer divided in centigrade degrees, so as to present a scale extending from $+35^{\circ}$ to -39°. It is now known that the aeronaut may encounter strata of air having a temperature lower than that of the congeation of mercury; hence the ordinary barometer will not answer, and an instrument should therefore be furnished, founded on the pressure exerted by the atmosphere on an elastic spring, and tested at very low temperatures under feeble pressures obtained by the pneumatic machine.

2. A vertical thermometer, of arbitrary graduation, the cylindrical reservoir of which is placed in the axis of several concentric envelopes of bright tin, open

at their bases to admit the circulation of air. This arrangement has been devised in order to obtain, at least approximately, the temperature which a thermometer would indicate in the shade.

3. Three thermometers, having arbitrary scales, attached to a metallic plate 5 centimeters apart. The reservoir of the first of these thermometers should have a vitreous surface; the surface of the second should be coated with lamp-black; and the reservoir of the third should be covered with a cylinder of polished silver, which must also envelop a portion of the stem. The reservoirs should be narrow cylinders, much elongated. Immediately below the reservoirs the metallic plate should support another plate brightly coated with silver. The plate bearing these thermometers should be arranged horizontally on one of the sides of the car, with a view to its remaining constantly exposed to the solar radiation.

4. A psychrometer formed by two thermeters of an arbitrary scale.

5. One of Regnault's condensing hygrometers.

6. Tubes of caustic potash, and also of pumice, wet with sulphuric acid, for the determination of the carbonic acid of the air. The air should be drawn in by means of a pump of the capacity of one litre (1.760 pint) accurately gauged.

7. Two balloons of one litre capacity, furnished with stop-cocks of steel, for collecting the air of the higher regions. These balloons, enclosed in tin boxes, should be scrupulously exhausted of air before the ascent.

8. A minimum thermometer of M. Walferdin, which should be enclosed in a tin cylinder, pierced with holes. It is best that this instrument should be placed under seal, as was done by MM. Barral and Bixio, since the control of the personal observations by means of a mute instrument imparts considerable value when they come to be verified, and affords a triumphant reply to objections which, through a natural tendency of the human mind, always oppose themselves to results which cannot be immediately verified by new experiments made under the same conditions. In the event, moreover, of the ascension of the balloon to heights where the temperature falls below $-40°$, the point of congelation for mercury, it will be necessary to have thermometers of alcohol or sulphuret of carbon, graduated below that point of the thermometric scale, so that the observations may not be interrupted by a circumstance which has ceased to be considered as of impossible occurrence.

9. It is from the considerations just stated, that I would recommend also the use of the apparatus devised by M. Regnault, and intended to indicate the minimum of barometric pressure, and consequently the maximum of elevation to which the balloon has attained. This apparatus should be enclosed in a tin case pierced with numerous small openings. The lid of this case should be secured with a seal like the minimum thermometer.

10. Polariscopic telescopes, such as I have described, *Astronomie populaire*, ii, p. 101.

11. Instruments for showing the declination, inclination, and intensity of magnetism, suspended in such a manner as not to be affected by the movements of rotation of the balloon in its ascent, as has been observed by MM. Biot, Gay Lussac, Barral, and Bixio.

12. Electrometers so constructed as to be capable of indicating at once the kind and the intensity of the electricity of different atmospheric strata.

It is scarcely probable that in an ascension, observers will be able to embrace at one time so many subjects of study, or use successively and opportunely so many instruments. The aeronaut should, on each occasion, limit himself to a small number of important inquiries. It is only in a series of aeronautic expeditions that a collection of records can be made corresponding to the great number of questions which the constitution of the terrestrial atmosphere presents for solution.

It is impossible to frame a programme which will embrace all the points worthy of examination; we are constrained to admit that the unforeseen will always play a principal part in aeronautic expeditions. We know little at present of the constitution of clouds, of the phenomena of refrigeration produced by their evaporation, of the mixture of strata of air differently saturated with humidity and derived from very different sources, of the action of electricity which traverses great aerial spaces, &c. In every case it is desirable that, during the progress of aerial voyages, there should be made, at least from hour to hour, in the principal terrestrial observatories, observations analogous to those which the aeronauts propose to undertake. This was advised in 1841 by a committee of the British Association, in a report relative to the advantages which science might derive from aerostatic ascensions, a report signed by Brewster, Herschel, Lubbock, Robinson, Sabine, Whewell, and Miller, and the advice was observed by MM. Barral and Bixio, who were thus enabled to connect the phenomena noticed in the higher regions of the air with those which occurred at the same time on the surface of Europe.

Barometric observations in connexion with those of temperature yield, by means of a formula which we owe to the genius of Laplace, the measure of the elevation to which balloons ascend above the level of the sea. This formula has been reduced into the usual tables which are found in the *Annuaire du bureau des longitudes.* The considerations on which the illustrious geometer founded his analysis led him to employ in his admirable formula a coefficient whose determination Ramond had arrived at, by comparing a great number of the measurements of the height of mountains taken with the barometer with their trigonometric measurements. Now, as Ramond operated chiefly under the parallel of 45°, and upon mountains whose elevation scarcely reached 3,000 meters, there is nothing to prove that the undetermined coefficient of Laplace's formula is susceptible of being applied to the measurement of much more considerable heights, and made in other latitudes. It would not be superfluous to measure directly, by observations made from several astronomical stations situated at known distances, the heights to which aeronauts attain, and to compare the results obtained with the barometric determinations. No doubt these operations will present numerous difficulties, and may be not unfrequently tried without success, because the balloons may disappear in the clouds or be carried in directions which will not permit the terrestrial telescopes to follow them with any advantage. But the problem to which I here call attention merits by its importance the sacrifices which may be encountered in giving it a satisfactory solution.

III.—AERONAUTIC VOYAGES OF LHOEST, ROBERTSON, AND SACHAROFF.

The first aeronautic voyage to which science was indebted for some useful indications was that performed at Hamburg, July 18, 1803, by the physicist, Robertson, accompanied by his countryman, Lhoest. They remained suspended in the air five hours and a half, and descended at Hanover, twenty-five leagues distant from the place of departure.

At the moment of the ascension the barometer on the earth stood at 28 inches, and the thermometer at $+16°$ Reaumur; at the greatest height to which they attained the barometer showed 12.4 inches, and the thermometer $-5°.5$ Reaumur. These observations, reduced to metric and centigrade measurements, give 758 millimeters for the barometric height, and $+20°$ for the temperature at starting; 336 millimeters and $-6°.9$ at the highest point reached. Hence, according to the formula of Laplace, we deduce 6,831 meters as the maximum height to which the balloon ascended.

The two aeronauts thought that at that height they observed the oscillations of the magnetic needle to be much less rapid than at the surface of the earth, and that consequently the magnetic intensity diminishes rapidly as the elevation

in the atmosphere increases. They also reported that they had experienced much physical suffering, and observed physiological phenomena, such as the swelling of the lips and veins, the bleeding of the eyes, &c., which have not been uniformly verified in subsequent expeditions.

However this might be, the Academy of Sciences of St. Petersburgh determined on a repetition of the experiment to be made by Robertson himself, assisted by Sacharoff, one of its own members, distinguished both as a physicist and chemist. This second expedition took place June 30, 1804. The aeronauts ascended from St. Petersburgh at 7 hours 45 minutes p. m., and descended at 10 hours 45 minutes, near Sivoritz, at a distance of about 20 leagues. At the moment of departure the barometer stood at 30 inches, and the thermometer at 19° Reaumur; at the greatest elevation the two instruments indicated respectively 22 inches and 4°.5 Reaumur. We conclude from these observations that the barometric pressure and the temperature were, at the point of departure, 812.1 millimeters, and +23°.7; at the greatest elevation, 595.5 millimeters and +5°.6; and from this it results that the highest point reached was 2,703 meters. MM. Robertson and Sacharoff were not able to make regular magnetic observations, but they felt authorized to affirm that the needle of declination had ceased to be horizontal, and that its north pole was elevated about 10 degrees, its south pole having an inclination of the same amount towards the earth.

IV.—VOYAGES OF BIOT AND GAY LUSSAC.

Saussure, after a series of observations made on the Col du Geant at a height of 3,435 meters, conceived it to be ascertained that at that height the magnetic intensity undergoes a sensible diminution, which he estimated at about one-fifth. This result appeared to be verified by the aeronautic voyages of Robertson, Lhoest, and Sacharoff, just spoken of. But the proofs of the fact were not given in a sufficiently decisive manner to secure it a definitive reception into science, and the question appeared important enough to the principal members of the Institute, Laplace, Berthollet, Chaptal, to justify a special experiment. This was intrusted to MM. Biot and Gay Lussac, who ascended from the garden of the *Conservatoire des arts et metiers*, August 24, 1804, provided with all the necessary instruments. The small dimensions of the balloon did not allow the two aeronauts to reach the height of more than 4,000 meters, and at that elevation the temperature, which had been +17°.5 on the earth, had only sunk to +10°.5. Leaving at 10 o'clock in the forenoon, they descended, about half after one, 18 leagues from Paris, in the department of Loiret. Taking advantage of the moments when the movement of rotation of the balloon in one direction stopped, being about to be resumed in the opposite direction, the learned physicists were able to determine the duration of five oscillations of the magnetic needle in different aerial strata, and they obtained the following results:

Heights.	Duration of 5 oscillations.
0 meters	35.25 seconds.
2,862 "	35 "
2,897 "	35 "
3,038 "	35 "
3,589 "	34 "
3,665 "	35.5 "
3,742 "	35 "
3,845 "	36 "
3,977 "	35 "

Thus the observations agree in giving 35 seconds for the duration of five oscillations, or at least the observed differences are too small to allow of any conclusion being drawn from them.

Under these circumstances it was evident that a new ascension ought to be undertaken. This time Gay Lussac ascended alone. He rose from the garden of the Conservatory, September 16, 1804, at 9 hours 40 minutes in the morning. He alighted at 3 hours 45 minutes, between Rouen and Dieppe, 40 leagues from Paris, near the village of Saint Gourgon.

The distinguished savant had furnished his aerostat with long cords, designed to moderate its movement of rotation, and he could consequently count more easily the oscillations of the magnetic needle; he obtained the following results:

Heights.	Duration of 10 oscillations.
0 meters	42.16 seconds.
3,371 "	41.5 "
3,857 "	42.0 "
4,551 "	42.5 "
4,294 "	41.8 "
4,367 "	43.0 "
4,765 "	42.2 "
4,848 "	42.8 "
5,277 "	42.2 "
5,671 "	42.5 "
6,146 "	42.0 "
6,182 "	41.0 "
6,923 "	41.7 "

From these observations, which do not present sufficiently appreciable differences, Gay Lussac drew the conclusion that the magnetic force does not undergo sensible variations up to the greatest heights which we can attain. In regard to this he thus expresses himself: "The consequence which we have drawn from our experiments may seem a little too precipitate to those who remember that we have not been able to make observations on the inclination of the magnetic needle. But when it is remarked that the force which causes a horizontal needle to oscillate is necessarily dependent on the intensity and direction of the magnetic force itself, and that it is represented by the cosine of the angle of inclination of this last force, the conclusion which we have arrived at cannot fail to be drawn, that, since the horizontal force has not varied, the total force cannot have varied, unless one chooses to suppose that the magnetic force may vary precisely in an opposite direction, and with the same relation to the cosine of its inclination, which is not at all probable. We have, moreover, in support of our conclusion, the experiment of the inclination which was made at the height of 3,902 meters, and which proves that at that elevation the inclination did not vary in a perceptible degree." This conclusion was logical at an epoch when it was not generally known that at a given place and under given circumstances the duration of the oscillations of a magnetic needle is influenced by its temperature. Now, the depression of the thermometer of Gay Lussac had been sufficiently considerable to produce noticeable changes in the magnetic needle. We see that, in the imperfect state of the instruments and the science in 1804, it was impossible to arrive at an exact solution of the problem which the Institute had in view. Even at present this problem is still unsolved.

The principal result of the aeronautic voyage of Gay Lussac relates to the constant composition of the atmospheric air to a height of 7,000 metres. The illustrious physicist had the good fortune to bring the first air from those high regions, and to give an analysis of it, whose accuracy has been uniformly verified by new experiments conducted with the improved processes which science has discovered during half a century.

Another fact, no less important, is the wide difference which Gay Lussac found between the temperatures below and at the great height to which he

ascended. At the moment of his departure the barometer registered 765.25 millimeters, and the thermometer $+27^\circ.75$; at the greatest elevation these instruments gave 328.8 millimeters for the pressure, and $-9^\circ.5$ for the temperature. It results that Gay Lussac rose to the height of 7,016 meters above the mean level of the sea, and that he found himself exposed to temperatures differing by 37°.

I shall not speak of the hygrometrical observations, because it only results from them, as from the greater part of those which have been made to this day, that the dryness of the air becomes very considerable in high regions of the atmosphere. Hair hygrometers are instruments whose indica ions are so little comparable with one another that it is impossible to deduce precise conclusions from them.

Gay Lussac has reduced to their just value the recitals of physical sufferings which are supposed to be felt in very elevated strata of air; he expresses himself on this subject with perspicuity and simplicity: "Arrived at the highest point of my ascension, 7,016 meters above the mean level of the sea, my respiration was sensibly embarrassed; but I was still very far from experiencing a degree of inconvenience which could induce me to descend. My pulse and respiration were much accelerated; and, breathing thus rapidly in an air of extreme dryness, I could not be surprised at having the throat so dry that it was painful for me to swallow bread."

It is thus seen that the ascensions of Biot and Gay Lussac are the first which have been made with marked success as regards the solution of scientific questions.

V.—VOYAGES OF BARRAL AND BIXIO.

MM. Barral and Bixio made two aeronautic voyages, by the last of which, especially, science was enriched with unforeseen results of great importance.

In reporting to the Academy of Sciences an account of the first excursion of these intrepid physicists, I expressed myself in nearly the following terms: "MM. Barral and Bixio had conceived the idea of ascending to a great height in order to study, with the improved scientific instruments of the present day, a multitude of atmospheric phenomena still imperfectly known. It was proposed to determine the law of the decrease of temperature with the height; the law of the diminution of humidity; to ascertain whether the chemical composition of the atmosphere is the same throughout; the portion of carbonic acid at different elevations; to compare the calorific effects of the solar rays in the highest regions of the atmosphere with these same effects observed on the surface of the earth; to determine whether there arrives at a given point the same number of calorific rays from all points of space; whether the light reflected and transmitted by clouds is or is not polarized, &c.

The instruments necessary for so interesting an expedition had been prepared with great care and precision by M. Regnault. Never has the love of science been manifested with more disinterestedness. M. Walferdin furnished several of his ingenious thermometers. The explorers were, besides, provided with barometers very accurately graduated, for determining the height at which the different observations were made.

The aeronauts had intrusted the preparation of the balloon to M. Dupuis Delcourt, who had distinguished himself by twenty-eight aerial voyages. All the arrangements were made in the garden of the Observatory of Paris. The ascension took place Saturday, June 29, 1850, at 10 hours 27 minutes in the morning, the balloon having been filled with pure hydrogen gas procured by the action of chlorhydric acid on iron.

According to all previous calculation, the explorers might now have expected to rise to the height of 10,000 or 12,000 meters, supposing the upper strata of the atmosphere to correspond with received theoretical ideas.

At the moment of departure, however, it might easily be seen that in several respects the aerostatic apparatus was imperfect. The balloon, in consequence of the prevalence of high winds, had been torn at many points, and mended with too great haste; the rain fell in torrents. What was to be done? It had been most prudent, perhaps, not to ascend, but the aeronauts rejected the idea. They placed themselves in the car, and boldly launched into the air, without even taking the precaution, so violent was the wind, of determining with a balance the ascensional force of the aerostat. Their ascent was extremely rapid; the spectators compared it to that of an arrow; they very soon disappeared in the clouds, and it was above the curtain which thus shrouded them from the view of man that the stirring scenes took place which remain to be described.

The dilated balloon pressed with great force on the meshes of the netting, which was much too small. It expanded from above downwards; descended on the aeronauts, whose car was suspended by cords which were too short, and covered them in some sort like a hood. At this time the adventurers found themselves in a situation of the greatest difficulty; one of them, in his efforts to disengage the cord of the valve, caused an opening in the inferior prolongation of the balloon; the hydrogen gas, which escaped nearly on a level with their heads, almost suffocated them, and caused excessive vomitings and momentary syncope.

Consulting the barometer, they found that they were descending rapidly, and, in seeking to ascertain the cause of this unexpected movement, they discovered that the balloon was torn in the region of its equator to the extent of nearly 2 meters. They now perceived, but with a composure which merits admiration, that all they could hope was to escape with life. It is no little to say that the velocity of their descent was much greater than that of their ascent. They discharged all their remaining ballast, threw overboard even the coverings which had been provided against the cold, including their furred boots, but parted with none of the instruments of research.

They fell, at 11 hours 14 minutes, in a vineyard, the ground of which was fortunately soft, in the commune of Dampmart, near Lagny. The laborers and vine-dressers ran to their help, and found the two aeronauts clinging by the feet and hands to the stems of the vines, in order to counteract as far as possible the horizontal movement of the car. The most earnest assistance was rendered them.

From a voyage performed under such conditions it is evident that science could derive but a very small amount of information in comparison with what might have been expected; yet it is our duty to say that our two physicists established, by decisive experiments, that the light of clouds is not polarized; that the bed of clouds which they traversed was at least 3,000 meters in thickness, and that, notwithstanding the existence of this curtain between the earth and sky, the decrease of temperature was very nearly the same with that verified by Gay Lussac in his celebrated voyage performed in a perfectly cloudless sky. From the barometrical observations compared with those made at the Observatory of Paris, it is deducible that, in the region where the balloon was torn, the height attained was 5,900 meters, and from a similar computation that the upper surface of the cloud passed through was at the height of 4,200 meters.

The following numbers complete the details which I laid before the Academy: At the moment of departure the barometer of the Observatory, reduced to zero, marked 753 millimeters, and the exterior thermometer 30°.3; the direction of the wind was west-southwest, and the sky was completely covered. At 10 hours 29 minutes the voyagers penetrated into a cloud having the appearance of a dense mist, which deprived them of the sight of the earth. At 10 hours 47 minutes the barometer of the car, reduced to zero, marked 458.3 millimeters, and

the thermometer $+7^\circ$; at the same instant the barometer of the Observatory indicated a pressure of 753.17 millimeters, and the thermometer $+ 19^\circ.4$. These numbers give, by calculation, the height of 4,242 metres above the mean level of the sea, and correspond with the moment at which the balloon emerged from the upper part of the clouds. The bed of clouds now below the observers presented the appearance of mamillary swellings, silver white in color, the light from which, examined with the polariscopic telescope, yielded no trace of polarization. Except a few clouds, which here and there rose high above the balloon, the sky was of a pale and dull blue. At 10 hours 59 minutes the barometer of the car indicated 373.4 millimeters, and the thermometer had sunk below zero. M. Barral was unable to make out the exact thermometric degree on account of a layer of hoar-frost deposited on the instrument, which he could not remove. The barometer was at this time in a state of oscillation, the mean height of its changes being represented by the number just mentioned. The balloon, which, notwithstanding the precise directions given, had been so constructed as not to leave sufficient room for the development incident to the natural dilatation of the hydrogen,* had now sunk down upon the excursionists; the valve provided for the escape of the gas was closed; a rent had taken place in the upper part of the balloon, and MM. Barral and Bixio fell to the earth after having traversed 5,800 meters in from four to five minutes.

They immediately commenced preparations for a new ascent, which took place a month after that of which an account has been given. They rose, as before, from the garden of the Observatory; and I was a witness of this, as I had been of their former ascension. I had taken part in all the deliberations which regarded the scientific purposes of the voyage. If the first one had been rendered, by unfavorable circumstances, almost entirely barren of results, beyond giving proof of the intrepidity of the two distinguished explorers, and initiating them in the dangers of an ascent through an atmosphere agitated by winds and turbid with thick clouds, it would be sufficient to read the journal of the second voyage to comprehend how fertile it was both in novelty and interest. The Academy of Sciences having judged it desirable that such a statement should be prepared as would enable those least familiar with these matters to appreciate the importance of the contribution made by MM. Barral and Bixio to meteorology, I yielded to the wishes of that learned body, and shall here reproduce, in nearly identical terms, the account of the voyage which I then submitted:

"The two scientific explorers having properly resolved to renew their enterprise under more favorable circumstances, and being no longer under a necessity of evincing their courage or punctuality, could afford to await patiently the day and the moment. M. Regnault took charge, with M. Barral, of the preparations, which is equivalent to saying that the utmost ingenuity and exactness presided over the construction and disposal of the instruments. No one, however, but an eye-witness, can appreciate the indefatigable zeal and devotedness which my distinguished colleague exerted day and night in this behalf.

"Everything was ready on Friday, July 26, 1850, but the weather was adverse. Saturday morning, the atmosphere having cleared up, the filling of the balloon was begun. The operation was tedious, and by the time it was finished, towards one or two o'clock, the sky was overclouded, and a deluge of rain was falling. The rain finally ceased, but the sky remained entirely overcast; it would have been only natural, under these circumstances, to

* The difficulty of managing the balloon before its ascent was the reason why the length of the cords attaching the car was reduced. The wind was so violent that 120 soldiers could scarcely keep the balloon from being carried away.

renounce the proposed ascension. I made, in the presence of the two aeronauts, the observation that it might be very useful to know the decrease of the atmospheric temperature with the height when a continuous screen of clouds shuts from us the view of the sky.* Now it sometimes happens that the sky becomes clear of a sudden; in this case there must remain in the atmosphere traces more or less marked of the abnormal decrease of temperature of which the presence of the cloud had been the cause. The observations made in aerostatic ascensions, performed during clear weather, are not completely applicable to this special case. Besides, there are numerous occasions when we observe through openings in the clouds. When MM. Barral and Bixio arrived at the conclusion, from these considerations and others which it would be superfluous to mention, that their voyage might prove useful, they placed themselves in the car and launched into the air.

"All the details of this ascension are scrupulously given in the journal written at the time by the aeronauts, and the calculations were compared by M. Regnault with the indications of the sealed instruments carried in the expedition. I shall only advert here to the fact that at their greatest elevation our explorers experienced no uneasiness or embarrassment in their respiration; and that M. Bixio, who had suffered in his first voyage from acute pain in the ears, guarded against that annoyance by simply counterfeiting from time to time the act of deglutition, by which the air within and without the organ was maintained in a state of equal pressure. It may be added, that they encountered a mass of cloud of more than 5,000 meters in thickness, that they did not succeed in rising entirely above it, but at the height of about 7,000 meters (22,960 feet) were forced to commence an involuntary descent, the effect of a rent in the lower part of the balloon. They might, perhaps, by throwing out the last of their ballast, have prolonged their stay at the height which they had reached, but circumstances no longer permitting them to gather useful indications for science, they thought best not to struggle against the downward tendency of the apparatus.

"Let us speak now of the observations which they had an opportunity of making. When they had attained their highest station in the immense bed of cloud, an opening took place in the vaporous mass which surrounded them, through which the blue sky was apparent. The polariscope, directed towards this region, showed an intense polarization; on the contrary, there was none at all, when the instrument was pointed aside beyond the opening. This should not be regarded as a repetition of the experiment made in the first voyage, for then they observed the light reflected by the clouds, while now it was in the transmitted light that they verified the absence of all polarization.

"An interesting optical phenomenon was exhibited during this ascension. Before attaining the highest limit, the bed of cloud which enveloped the balloon, having diminished in thickness or become less dense, the sun appeared weak and quite white; at the same time there appeared, below the horizontal plane of the car, at an angular distance from that plane equal to the angle formed by the sun's height, a second sun similar to one which might have been reflected from a sheet of water situated at that elevation. It is natural to suppose, with our aeronauts, that the second sun was formed by the reflection of the luminous rays on the horizontal faces of crystals of ice floating in that vaporous atmosphere.

"We now come to the most striking and wholly unexpected result furnished by the thermometrical observations. Gay Lussac, in his ascension in clear or rather slightly vaporous weather, had found a temperature of 9°.5 below zero

* The refractions at moderate heights depend on the law according to which this decrease is effected.

at the height of 7,016 meters. This was the minimum he observed. MM. Barral and Bixio encountered this same temperature in the cloud at the height of about 6,000 meters; but from this point, through an extent of some 600 meters, the temperature varied in a manner the most extraordinary, and beyond all anticipation. Lest the number which results from the observations should strike the reader with a feeling of incredulity, it is proper to say that proof of its exactness will be promptly submitted. At the height of 7,049 meters, at some distance from the upper limit of the cloud, MM. Barral and Bixio saw the centigrade thermometer descend to 39 degrees below zero. It is 30 degrees lower than the number observed by Gay Lussac at about the same height, but when the weather was clear.

"I hasten to prove that this surprising result is affected by no error of observation. The barometer for determining the height was of course furnished with a thermometer intended to give the temperature of the mercury. This thermometer had been graduated to 37 degrees below zero. It was thought that these 37 degrees ought to suffice for the greatest heights to which it was supposed explorers could ascend. But the mercury had descended below this 37th degree, though it had not shrunk entirely within the reservoir. By an estimate which could hardly be inexact when made by such a physicist as M. Regnault, the mercury had descended 2 degrees below 37. The thermometer of the barometer marked, therefore, 39 degrees.

"M. Walferdin has invented very ingenious self-registering thermometers, which give the maxima and minima of temperature to which they have been exposed. The one for maxima is frequently used; it is desirable that the second, which is less known, should be generally adopted by physicists. It is capable of being of great service to meteorolgy. The inventor had sent one of his thermometers *á minima* with arbitrary divisions to our aeronauts, and this was enclosed in a case with numerous holes to permit the circulation of air. At the request of the two aeronauts, a seal was applied, and this seal, which arrived untouched, was broken at the College of France in the presence of MM. Regnault and Walferdin. Careful examination proved that the minimum thermometer had sunk to —39°.7. After these precise observations it is scarcely necessary to say that the proof of an extraordinary depression of temperature is to be found in the impossibility which the aeronauts experienced of reading the indications of several thermometers, the fluid of which had sunk as low as the stopper of cork which supported them. Every attempt to remove this obstruction was frustrated by the stiffening of the fingers with cold. This nearly instantaneous depression of the temperature in the cloudy mass is a discovery which interests meteorology in the highest degree. What is the special constitution of a cloud which qualifies it, whether by radiation into space or from whatever other cause, to exhibit so prodigious a refrigeration? It is a question which at this moment we can do no more than propound. Can this abnormal constitution play a part in the formation of hail? Is it, perchance, the cause of the considerable changes of temperature which are suddenly experienced at a given place? The solution of these questions is reserved for the future, which does not, however, at all diminish the importance of the observation.

"In the journal of the voyage the temperatures observed were rendered by thermometers having an arbitrary graduation; the aeronauts did not know what the numbers signified which they read and registered; the real temperatures were afterwards determined by M. Regnault, and the heights calculated by M. Mathieu. We may thus rely with perfect confidence on the results. From these we deduce that the height attained was 7,049 meters, taking into account the diminution of weight at those great elevations and the influence of the hour of the day on the barometric measurement of heights; this is 33 meters higher

than Gay Lussac had ascended. It is proper to observe that the formulas used in calculating heights proceed upon the hypothesis of a nearly uniform decrease of temperature, and that, in this instance, a change of elevation which may be estimated at 600 meters, was attended by a variation of temperature of about 30 degrees, while, in an unclouded atmosphere, the variation would have been but from 4 to 5 degrees.

"The important discovery made in this aeronautic voyage shows what science may expect from like expeditions when they shall be confided, as at that time, to intrepid, careful, exact, and candid observers."

The following is an extract from the journal kept by the two accomplished physicists during the voyage:

"The graduated instruments which we carried with us had been constructed by M. Fastré, under the direction of M. Regnault. The tables of graduation had been prepared in the laboratory of the College of France, and were known only to the last mentioned-savant.

"The balloon is the same which served for our first ascension; it is formed of two hemispheres of a radius of 4.8 millimeter, separated by a cylinder 3.8 millimeter in height, having for its base a great circle of the sphere. The total volume of the balloon is 729 cubic meters. A lower orifice, intended to give issue to the gas during its dilatation, is terminated by a cylindrical appendage of silk, 7 meters long, which is left open to permit the free escape of the gas during the period of ascent. The car is suspended at about 4 meters below the orifice of the appendage, so that the balloon may float at the distance of 11 meters from the car, and in no respect interfere with the observations. The instruments are fixed around a large cast-iron ring which is attached to the usual wooden circle for securing the cords of the car, and is of such a form that the instruments may be within convenient distance of the observers.

"It was our intention to set out at about 10 o'clock a. m., and measures had been taken for commencing the inflation of the balloon, an operation with which MM. Veron and Fontaine were charged, at 6 o'clock. Unfortunately, circumstances beyond our control, and arising from the necessity of thoroughly washing the gas in order to guard against its action upon the tissue of the balloon, occasioned delay, and it was 1 o'clock before the arrangements were completed. The sky, which had been quite clear till noon, became covered with clouds, and soon a deluge of rain was falling upon Paris. This continued until 3 o'clock. The day was then too far advanced, and the condition of the atmosphere too unfavorable, for us to hope that we could carry out the programme we had proposed. But the aerostat was ready, great expense had been incurred, and it was possible that observations in this troubled state of the atmosphere might lead to useful results. We decided, therefore, to ascend. Our departure took place at 4 o'clock. Some difficulty was occasioned by the narrowness of the space which the garden of the observatory afforded for the evolution of ascent. The balloon, as has been seen, was at a considerable distance from the car, and, swept forward by the wind, got the start of the frail skiff in which we were embarked, so that it was only through a series of oscillations, sufficiently divergent on either side from a vertical line, that we attained a state of tranquil suspension from the aerostat. We came in contact with trees and a pole, by which one of the barometers and the thermometer with a blackened surface were broken, and these were left behind. We shall here transcribe the notes taken during our ascension.

"4^h 3^m. *Departure.*—The balloon ascends at first slowly, taking a direction towards the east. The movement of ascension becomes more rapid after the discharge of some kilograms of ballast. The sky is completely covered with clouds, and we presently find ourselves in a light mist.

Hours.	Barometer.	Thermometer.	Height.
	Millimeters.		*Meters.*
4^h 6^m 6^s	694.70*	+16°	757
4 8 0	674.96	....	999
4 9 30	655.57	+13.0	1,244
4 11 0	636.68	+ 9.8	1,483

"Above us there extends an uninterrupted bed of clouds; below we perceive here and there detached clouds which appear to float over Paris. The wind is fresh.

Hours.	Barometer.	Thermometer.	Height.
4^h 13^m	597.73	+9°.0	2,013
4 15	558.70	....	2,567
4 20	482.20	—0 .5	3,751

"The cloud which we enter presents the appearance of an ordinary dense fog. The earth is no longer discernible.

Barometer.	Thermometer.	Height.
405.41	—7°.0	5,121 meters.

"A few solar rays become perceptible through the clouds.

"The barometer oscillates from 366.99 millimeters to 386.42 millimeters; the thermometer marks —9°.0; calculation gives from 5,911 to 5,492 as the height reached at this point of time.

"The balloon is entirely inflated. The appendage, compressed till now by the external atmosphere, is at present distended, and the gas escapes by its lower orifice under the form of a whitish trail; we perceive its odor very distinctly. We discover in the balloon, at the distance of about 1.5 millimeter from the insertion of the appendage, a rent, which affords issue to a greater amount of gas, without diminishing, however, in any important degree, the ascensional force of the aerostat.

"An opening in the cloud enables us vaguely to perceive the position of the sun.

"The balloon resumes its ascendant movement after a new discharge of ballast.

"4^h 25^m.—Oscillations of the barometer between 347.75 millimeters and 367.04 millimeters indicate a new station of the balloon; the thermometer varies

* All the barometric heights taken have been reduced to the temperature of 0° by calculation. By means of the barometric and thermometric observations, made at the observatory and in the car, the heights of nineteen stations above the observatory and above the sea were calculated, increasing them by 65 meters. But the three heights, 6,512, 7,049, and 6,765 meters, where the temperature had sunk to — 35°, — 36°, and — 39°, were obtained by calculating, not from the observatory, but from the intermediate station of 5,902 meters, where the temperature was — 9°.8, and the pressure 367.04 millimeters. There results 7,004 for the highest station. But it is still necessary to add a correction of 12 meters, due to the height (5,902 meters) of the inferior station of comparison, and 33 meters on account of the influence of the hour of the day, as was justly remarked by M. Bravais, which makes in all 7,049 meters.

from —10°.5 to —9°.8; the height we have reached varies from 6,330 to 6,902 meters.

"The fog, much less dense, permits our seeing a white and feeble image of the sun.

"A renewed discharge of ballast occasions a new ascension of the balloon, which attains a new stationary position, indicated by renewed oscillations of the barometer. We are covered with small particles of ice, in the shape of extremely fine needles, which accumulate in the folds of our clothing. While the barometric oscillation is descending, and the movement of the balloon is consequently ascensional, these particles fall upon our open note-book in such quantity as to produce a sort of crepitation. Nothing similar is observed while the barometer is rising and the balloon, of course, descending.

"The horizontal glass thermometer indicates —4°.69; the silver-plated thermometer —8°.95.

"We distinctly see the disc of the sun through the frozen mist; but at the same time, in the same vertical plane, we perceive a second image of the sun, almost as intense as the former. The two images appear symmetrically disposed above and below the horizontal plane of the car, each making with this plane an angle of about thirty degrees. This phenomenon is apparent for more than ten minutes.

"The temperature lowers rapidly. We prepare to make a complete series of observations on the thermometers of radiation and those of the psychrometer, but the mercurial columns are hidden by the stoppers, inasmuch as no such rapid fall in the temperature had been anticipated. The thermometer with concentric envelopes of tin gives —23°.79.

"We open a cage in which two pigeons are confined, but they refuse to escape. We cast them off into space, when, spreading their wings and wheeling in large circles, they sink downwards and are soon lost to sight in the mist which surrounds us. We cannot perceive the anchor which is suspended below, at the end of a cord 50 meters long.

"$4^h\ 32^m$.—We discharge ballast and rise still higher. The clouds separate above, and we see in the sky a space of bright azure blue, similar to that seen on earth in clear weather. The polariscope indicates no polarization, in any direction, on the clouds immediately around or remote from us. The blue of the sky, on the contrary, is strongly polarized.

"The oscillations of the barometer indicating that we have ceased to ascend, we throw out ballast, and obtain a new ascensional movement.

Hours.	Barometer.	Thermometer.	Height.
	Millimeters.		*Meters.*
$4^h\ 45^m$	338.05	—35°	6,512

"Our fingers are stiffened with cold, but we experience no pain in the ears, nor is respiration at all embarrassed. The sky is covered anew with clouds, but the sun, though veiled, is still seen, as well as its image. We think it would be interesting to find if the cold will still increase on ascending yet higher. We throw out ballast, which determines a further ascension.

"$4^h\ 50^m$.—The barometer marks 315.02 millimeters. The extremity of the column of the thermometer of the barometer is lower by about two degrees than the last division marked on the instrument. This division is —37°; the temperature, therefore, was about —39°; the height, consequently, which we had attained is 7,039 meters.

"The barometer oscillates from 315.02 millimeters to 326.20; hence the balloon oscillates from 7,039 to 6,798 meters. There are only four kilograms of

ballast remaining, which we judge it prudent to keep for the descent. Besides, it is useless to try to mount higher with instruments which have become mute; the mercury is congealed. We could, at most, only strive to maintain ourselves for some time at the same height, but, although the appendage is raised to prevent the escape of gas by its orifice, the balloon begins to descend. We proceed to secure portions of the air, and, though the tube of one of the receptacles is broken in attempting to turn the stop-cock, the other is filled without accident. But the cold paralyzes all our efforts; observations have become impossible; our fingers are disqualified for every operation. We resign ourselves to a descent.

Hours.	Barometer.	Temperature.	Height.
	Millimeters.		*Meters.*
$5^h\ 2^m$	436.40	—9°	4,502

"We still encounter the little needles of ice:

Hours.	Barometer.	Temperature.	Height.
	Millimeters.		*Meters.*
$5^h\ 7^m$	483.16	—7°	3,688
5 10	540.39	—3	2,796
5 12	559.70	—1	2,452
5 14	582.90	0	2,185

"The thermometer with a glass surface marks +2°.50; that with a silvered surface +1°.91.

"$5^h\ 16^m$.—The barometer oscillates from 598.5 millimeters to 618.0 millimeters, because we throw over our ballast, and this arrests our descent; the temperature is 1°.8; the height varies from 1,973 to 1,707 meters.

"The oscillations are prolonged by the discharge of the last portions of our ballast. We are now only occupied with moderating the descent by sacrificing all that we have at our disposal, except the instruments, and we place the thermometers in their cases.

"$5^h\ 30^m$.—We touch the earth at the hamlet of Peux, a commune of Saint Denis les Rebris, arrondissement of Coulommiers, (Seine et Marne,) at some paces from the residence of M. Brulfert, mayor of the commune, 70 kilometers distant from Paris.

"We had the good fortune to break no instrument in our descent. The village afforded but a single vehicle to carry us to the Strasbourg railroad, 18 kilometers distant, and the transfer was rendered troublesome by a violent storm of wind and rain; the horse fell, breaking two of the instruments, which we greatly desired to carry safe to Paris, namely, the balloon for air, and the instrument indicating the minimum of barometric pressure. Fortunately the minimum thermometer of M. Walferdin, with his seal, was conveyed intact to the College of France. Here the seal was removed by MM. Regnault and Walferdin, and the minimum of temperature determined, by direct experiment, was found to be —39°.67, consequently very little different from the lowest temperature observed by ourselves on the thermometer of the barometer."

In rendering my report to the Academy of Sciences, I remarked that the fact of the presence of a cloud composed of small particles of ice having a temperature of about —40° in mid-summer, at a height of from 6,000 to 7,000 meters above the surface of Europe, is the greatest discovery which meteorology has

for a long time recorded. This discovery explains how these icy particles may become the nucleus of hailstones of considerable volume, for we readily comprehend how they may condense around them and solidify the aqueous vapors contained in the atmospheric strata in which they float; it likewise demonstrates the truth of the hypothesis of Mariotte, who attributed the existence of halos, parhelia, and paraselenes, to crystals of ice suspended in the air. In fine, the presence of a widely-extended cloud of great coldness very well accounts for the sudden changes of temperature which so often and unexpectedly affect our climates. MM. Barral and Bixio, in discussing the meteorological observations made in Europe at the time, including the day preceding and the day following their memorable ascension, were enabled to establish the occurrence of sudden and general accessions of cold, which bore undoubtedly a direct relation to the arrival of the intensely frigorific masses of vapor which were then propagating themselves from the northeast to the southwest.

VI.—VOYAGES OF JOHN WELSH.

In July, 1852, the committee of directors of the observatory of Kew, near London, resolved on the execution of a series of aeronautic ascensions with a view to the investigation of the meteorological and physical phenomena which develop themselves in the most elevated regions of the terrestrial atmosphere. This resolution was approved by the council of the British Association for the Advancement of Science. Instruments were immediately prepared, consisting of a barometer of Gay Lussac, dry and wet thermometers, an aspirator, a condensing hygrometer of Regnault, a hygrometer of Daniell, a polariscope and glass tubes to collect the air. The balloon made use of was that of M. Green, who constantly accompanied M. John Welsh, to whom the observations were intrusted; illuminating gas was employed for inflation. Four ascensions took place, August 17 and 26, October 21, November 10, 1852. In the first two voyages M. Nicklin also accompanied M. Welsh. The place of departure was the garden of Vauxhall.

In the first ascension, August 17, the expeditionists set forth at 49 minutes after three in the evening, and again touched the earth at 20 minutes after five, 23 leagues north of London. They reached the height of 5,947 meters. The lowest pressure they obtained was 364.5 millimeters, and the minimum temperature $-13°.2$. On the earth the barometer indicated 755.1 millimeters, and the thermometer $+21°.8$. A cloud covered the horizon, its inferior limit was reached at about 762 meters, and its superior limit at 3,963 meters. The balloon then penetrated into pure air, but at a great distance above there spread a dense cloudy mass. Snow, consisting of star-shaped flakes, fell from time to time on the balloon.

The second ascension, August 26, commenced at 4 hours 43 minutes in the evening, and terminated at 7 hours 35 minutes; the descent took place 10 leagues W.NW. of London. The balloon rose to a height of 6,096 meters, and the lowest temperature observed was $-10°.3$. On the earth the pressure was 760.9 millimeters, and the temperature $+19.1$. A few clouds were suspended in the atmosphere at a height of about 900 meters; above, the sky was clear and of a bright blue.

The third ascension took place October 21, at 2 hours 45 minutes; the voyagers descended at 4 hours 20 minutes, about 12 leagues to the east of London. They ascended only to a height of 3,853 meters; the least pressure observed was 475.5 millimeters, the lowest temperature $-3°.8$. On the earth the barometer marked 759.2 millimeters, the thermometer $+14°.2$. Between 254 and 853 meters, the balloon encountered detached and irregular clouds; at about 915 meters it entered a continuous bed of cloud, whose upper surface terminated at 1,093 meters. On its emergence from the cloud the balloon projected on its nearly level expanse a shadow surrounded with fringes. The light, directly

reflected by the cloud, examined with the polariscope, presented no trace of polarization.

The greatest height at which M. Welsh arrived was attained in his fourth voyage, performed the 10th of November. The ascent took place at 2 hours 21 minutes, and the descent, near Folkstone, 23 leagues E.SE. of London, at 3 hours 45 minutes. The height reached was 6,989 meters; minimum temperature observed —23°.6; minimum pressure 310.9 millimeters. On the earth the barometer indicated 761.1 millimeters, and the thermometer +9°.6. A first cloud was encountered at 254 meters, whose upper surface reached a height of 600 meters. There occurred next a space of 620 meters, free from all sensible vapor; but, at a height of 1,220 meters, a new cloud was met with, which terminated at 4,494 meters. Beyond this there were only a few cirri at a very great height.

We see that the English aeronauts only once approached, though without attaining, the height of 7,000 meters, reached by Gay Lussac, and by Barral and Bixio. The very low temperature of —23°.6, observed by Welsh in his last ascension, would certainly have appeared extraordinary if our countrymen, in their expedition of July 27, 1850, had not encountered a cloud having a much lower temperature. The air collected by Mr. Welsh was analyzed by M. Miller, who found its composition the same with that of normal air. The hygrometrical observations which Mr. Welsh made with care, and in great number, by help of the psychrometer and hygrometer of M. Regnault, did not indicate any considerable dryness. On the contrary, even in the highest regions, the relative atmospheric humidity approached saturation.

VII.—THE GREATEST HEIGHTS REACHED, AND THE TEMPERATURES OBSERVED, IN THE UPPER REGIONS OF THE ATMOSPHERE.

It is worthy of remark that, to the present time, man has not ascended into the atmosphere as high as the aerial stratum which surrounds the loftiest mountain summits of the Old and New World. Kintschindinga and Aconcagua, the former 8,592, the latter 7,291 meters high. In the ascent of mountains, barely 6,000 meters may be assigned as the height to which human effort has attained. In June, 1802, my illustrious friend, Alexander Humboldt, accompanied by M. Bonpland, ascended Chimborazo to the altitude of 5,878 meters. In December, 1831, another of my friends, M. Boussingault, accompanied by Colonel Hall, climbed the same mountain to the height of 6,004 meters above the level of the sea. If we add to these two celebrated excursions the aeronautic voyages of Lhoest and Robertson, July 18, 1803; of Gay Lussac, September 16, 1804; of MM. Barral and Bixio, July 27, 1850; of M. Welsh, August 26 and November 10, 1852, we have the sum of all the enterprises in which man has succeeded in maintaining his position for a few instants in the strata of air situated from 6,000 to 7,000 meters above the mean level of the seas. The following table recapitulates the thermometric and barometric observations made under these rare circumstances:

Names.	Dates.	Greatest heights attained.	Lowest barometric pressures observed. (Reduced to 0°.)	Lowest temperatures observed.
		Meters.	*Millimeters.*	*Degrees.*
Humboldt and Bonpland........	June 24, 1802	5,878	376.7	— 1.6
Lhoest and Robertson..........	July 18, 1803	6,831	336.0	— 6.9
Gay Lussac....................	Sept. 16, 1804	7,016	328.8	— 9.5
Boussingault and Colonel Hall..	Dec. 16, 1831	6,004	371.1	+ 7.8
Barral and Bixio..............	July 27, 1850.	7,049	315.0	—39.7
Welsh.........................	Aug. 26, 1852	6,096	371.1	—10.3
Welsh.........................	Nov. 10, 1852	6,989	310.9	—23.6

These figures certainly demonstrate that, in high atmospheric regions, the thermometric variations are not less considerable than on the surface of the earth, and that, in any case, if there be a stratum of constant temperature in the terrestrial atmosphere, the fact of its existence is only admissible as regards an elevation probably much greater than any yet reached. Is it practicable to transcend this limitary height of 7,000 meters, by which all ascensions hitherto undertaken have been bounded? There is but one consideration which can make us hesitate to answer affirmatively. We know not if man's physical constitution could adapt itself to a pressure much lighter than that of 311 millimeters, about two-fifths of the mean pressure observed on the sea-shore.

AN ACCOUNT OF BALLOON ASCENSIONS.

BY MR. JAMES GLAISHER.

[*From the London Athenæum, October*, 1864.]

THE committee on balloon experiments was appointed last year for the following purposes: To examine the electrical condition of the air at different heights; to verify the law of the decrease of temperature; and to compare the constants in different states of the atmosphere. With respect to the first of these objects no progress had been made, with the exception of preparing an instrument and apparatus for the investigation. At the request of the committee Mr. Fleming Jenkin undertook the construction of the best instrument for the purpose, and one was finished towards the end of 1863, but it was constructed to be used with fire. It has since had to be adapted for water, a constant flow of which is necessary in electrical experiments in balloons. This apparatus Mr. Glaisher was requested by the committee not to use, as they felt that these instruments, if exerting no influence while the balloon was rising, might, when it was falling, throw considerable doubt on the experiments relating to humidity. With respect to the second of these objects, the verifying the law of the decrease of temperature in different states of the atmosphere, the committee considered would be best attained by taking as many observations as possible at times in the year, and at times in the day, at which no experiments had been made, for the purpose of determining whether the laws which hold good at noon apply equally well at all other times of the day. The committee have always pressed the importance of magnetic observations in the higher regions of the air—the Astronomer Royal suggesting the use of a horizontal magnet, and taking the times of its vibration at different elevations, a method which is seldom practicable, owing to the almost constant revolution of the balloon. To obviate this, Dr. Lloyd suggested the use of a dipping-needle, placed horizontally when on the ground, by means of a magnet above it, so that, when in the balloon, the deviation from horizontality might be noticed, and which deviation would be independent of rotary motion of the balloon. The latter method has not yet been tried, Dr. Lloyd wishing some experiments to be made before the instrument was constructed. At Newcastle a very general wish being expressed that very high ascents should not again be attempted, none above five miles had since been made. Mr. Glaisher then gave an account of the ascents made by him during the past year. The first was from Newcastle, on the 31st of August. The balloon left the earth at 6h. 11m. p. m., with a north wind, and descended at five minutes past 7, at Pittington, near Durham. The decrease of temperature within the first 200

feet of the earth in this ascent was very remarkable, no such rapid decrease having been found in any other ascents. On the ground the temperature was 64°, and by the time 200 feet had been attained, a decrease of 8 degrees had taken place, the temperature being 56°. From this height to 1,200 feet there was but little change, and above this the temperature decreased from 2° to 3½° in each succeeding 1,000 feet up to 7,000 feet, when the balloon entered a relatively warmer current of air. The second ascent, on the 29th of September, 1863, was from Wolverhampton. The gas on this occasion had been prepared in July expressly for a high ascent intended to have taken place before the Newcastle meeting, but circumstances prevented this being made, and the gas was obligingly stored in the gasometer by the directors of the gas-works. The balloon left at 7h. 43m. a. m., wind SW. At 8,200 feet there were two layers of clouds below the balloon and very dense clouds above. When at 11,000 feet the clouds were still a mile higher; there was a sea of blue-tinged cloud below, and peeps of the earth was seen through the breaks. At 13,000 feet high clouds were still above; but after this they began to dissipate, and at 9h. 38m., at 14,000 feet, the sun shone brightly. Ten minutes afterwards the travellers discovered the Wash at a distance of only ten miles, and were compelled to descend. A southwest gale was blowing, and so strong was the wind that on the grapnels taking the ground near Sleaford, at 10h. 30m., the balloon was rent from top to bottom. In this ascent warm currents were met with at 8,000 and 13,500 feet. In the descent a warm current was passed through, extending from 14,000 to 9,000 feet. Temperature at the ground on leaving 48°; at time of descent 53°. On passing out of the mist at 3,000 feet the humidity declined to 58° at 8,000 feet. Here there were dense clouds above and below. At 9,000 feet the humidity was 71°, and then the air became suddenly dry. The third ascent was made from the Crystal Palace, at 4h. 29m. p. m., on the 9th of October. In seventeen minutes it was 7,300 feet high, and directly over London Bridge, and all the vast number of buildings, comprising the whole of London, could be clearly seen. There were neither warm nor cold currents met with on this day. The secretary of state for war having granted permission to the committee to avail themselves of the facilities afforded in the Royal Arsenal, at Woolwich, the ascent of the 12th of January was made from thence. It was intended to have been made on the 21st of December previous, and from time to time the balloon had been partially inflated. It left at 2h. 7m. p. m., and in 14 minutes had crossed the Tilbury railway, and was over Hainault forest. At 3h. 31m. the height of 12,000 feet was attained, when the balloon began to descend, and touched the ground at 4h. 10m. at Lakenheath. On the earth the wind was SE. At 1,300 feet a strong SW. current was entered, in which the balloon continued up to 4,000 feet, when the wind changed to S. At 8,000 feet the wind changed to S.SW., and afterwards to S.SE. At 11,000 feet fine granular snow was met with, and the balloon passed through snow on descending till within 8,000 feet of the earth. Clouds were entered at 7,000 feet, which merged at about 6,000 feet into mist. This ascent is the only one ever made in January for scientific purposes. The fifth ascent was designed to have been made as near the 21st of March as possible, but through adverse weather was deferred to the 6th of April. The balloon left Woolwich at 4h. 7m. p. m., with a SE. wind, ascending evenly at the rate of 1,000 feet in about three minutes, till 11,000 feet was attained at 4h. 37m. It descended into Wilderness Park, near Sevenoaks, in Kent. Its course was most remarkable, having passed over the Thames into Essex. The balloon, unknown to the aeronauts, must have repassed the river and moved in a directly opposite direction, and so continued till it approached the earth, when it again moved in the same direction as at first. The ascent is remarkable for the small decrease in temperature with increase of elevation. The air, at the period of starting, was 45½°, and did not decline at all till after reaching 300 feet, after which it decreased gradually to 33° at 4,300. A warm current was then en-

tered, and the temperature increased till 7,500 feet was attained, when 40° were attained, being the same as had been experienced at 1,500 feet. It then decreased to 34° at 8,800 feet, and then increased slowly to 37° at 11,000 feet, a temperature which had been experienced at the heights of 8,500, 6,500, and 3,000 feet in ascending. After the great injury to the balloon on the 29th of September, in addition to the repairs it had previously undergone, Mr. Coxwell did not consider it, after the additional rough usage in the last two voyages, safe for extreme high ascents, and determined to build a new one, which he did, capable of containing 10,000 cubic feet more gas than the old one, so that, if need be, two observers could ascend together to the height of five miles. A new balloon, however, needs trying in low ascents until it proves gas-tight before it can be used for great elevations; and, on June 13, it was therefore started on a small ascent from the Crystal Palace, at 7 o'clock—the sky cloudless, and the air perfectly clear, except in the direction of London. An elevation of 1,000 feet was reached in 1¼ minute, 3,000 feet at 7h. 8m., when the balloon descended to 2,300 feet, and then reascended to 3,400, when, after a slight dip, it again ascended to 3,550 feet, the highest point by 7h. 28m., and then, after some oscillations, began its downward course at 7h. 50m. from 2,800 feet, reaching the ground at East Horndon, five miles from Brentwood, at 8h. 14m.—the remarkable feature in this voyage being that, below 1,800 feet elevation, there was scarcely any change of temperature until the earth was reached. This fact of no change in the temperature of the air at the time of sunset was very remarkable, for it indicated that, if such be a law, the law of decrease of temperature with increase of elevation may be reversed at night for some distance from the earth. June 20, the balloon left Derby at 17 minutes past 6 p. m., and descended near Newark. June 27, the balloon ascended from the Crystal Palace at 6h. 33½m.—the sky cloudy, wind west. The descent was made on Romney Marsh, 5 miles from the shore. These several trial trips of the new balloon were made, and it was gradually becoming gas-tight, when its lamentable destruction at Leicester took place. The mayor of that town has recently presided over a meeting for the purpose of collecting subscriptions to assist Mr. Coxwell to rebuild a new balloon; and we concur in Mr. Glaisher's wish that the town of Leicester and the Foresters' Society will soon remove the stigma resting upon them. Mr. Coxwell, since then, has had recourse to the old balloon, which he had repaired as best he could, and the next and last ascent of which Mr. Glaisher had to speak was made with it, on August 29, from the Crystal Palace, at 4h. 6m. The difference between the temperatures of the air and those of the dew-point in this ascent was rather remarkable. The most important point in the past year's experiments are that, though the law of decrease of temperature under ordinary circumstances in the summer months is pretty well determined, we cannot say such a law holds good throughout the year; nor can we say that the laws which are in force during the day will be in force at night. In carrying out these experiments Mr. Glaisher said he had freely given up all his leisure, and that Mr. Coxwell had done the same in a most unselfish manner. Indeed, had it not been for the generous spirit in which Mr. Coxwell had entered into these experiments, they never could have been made, except at a multiple of the cost that had been incurred.

AN ACCOUNT

OF

THE ABORIGINAL INHABITANTS

OF

THE CALIFORNIAN PENINSULA,

AS GIVEN BY

JACOB BAEGERT, A GERMAN JESUIT MISSIONARY, WHO LIVED THERE SEVENTEEN YEARS DURING THE SECOND HALF OF THE LAST CENTURY.

TRANSLATED AND ARRANGED FOR THE SMITHSONIAN INSTITUTION BY CHARLES RAU, OF NEW YORK CITY.

INTRODUCTION.

WHEN, in 1767, by a decree of Charles III, all members of the order of the Jesuits were banished from Spain and the transatlantic provinces subject to that realm, those Jesuits who superintended the missions established by the Spaniards since 1697 in Lower California were compelled to leave their Indian converts, and to transfer their spiritual authority to a number of friars of the Franciscan order. One of the banished Jesuits, a German, who had spent seventeen years in the Californian peninsula, published, after his return to his native country, a book which contains a description of that remote part of the American continent, and gives also quite a detailed account of its aboriginal inhabitants, with whom the author had become thoroughly acquainted during the many years devoted to their conversion to Christianity. This book, which is now very scarce in Germany, and, of course, still more so in this country, bears the title: *Account of the American Peninsula of California; with a twofold Appendix of False Reports. Written by a Priest of the Society of Jesus, who lived there many years past. Published with the Permission of my Superiors. Mannheim*, 1773.*

Modesty, or perhaps other motives, induced the author to remain anonymous, but with little success; for his name, which was *Jacob Baegert*, is sometimes met with in old catalogues, in connexion with the title of his book. That his home was on the Upper Rhine he states himself in the text, but further particulars relative to his private affairs, before or after his missionary labors in California, have not come to my knowledge. He does not even mention over which of the fifteen missions existing at his time on the peninsula he presided, but merely says that he had lived in California under the twenty-fifth degree, and twelve leagues distant from the Pacific coast, opposite the little bay of St. Magdalen. On the map accompanying his work there are two missionary stations marked under that latitude—the mission of St. Aloysius and that of the

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* Nachrichten von der Amerikanischen Halbinsel Californien: mit einem zweyfachen Anhang Falscher Nachrichten. Geschrieben von einem Priester der Gesellschaft Jesu, welcher lang darinn diese letztere Jahr gelebet hat. Mit Erlaubnuss der Oberen. Mannheim, 1773.

Seven Dolors, (Septem Dolorum,) of which the first named evidently was his place of residence.

The work in question constitutes a small octavo volume of 358 pages, and is divided into three parts. The first division (of which I will give a short synopsis in this introduction) treats of the topography, physical geography, geology, and natural history of the peninsula; the second part gives an account of the *inhabitants*, and the third embraces a short but interesting history of the missions in Lower California. In the appendices to the work the author refutes certain exaggerated reports that had been published concerning the Californian peninsula, and he is particularly very severe upon *Venegas'* "Noticia de la California," (Madrid, 1757, 3 vols.,) a work which is also translated into the English, French, and German languages. He accuses the Spanish author of having given by far too favorable, and, in many instances, utterly false accounts of the country, its productions and inhabitants, which is rather a noticeable circumstance, since *Venegas* is considered as an authority in matters relating to the ethnology of California.

While reading the work of the German missionary, I was struck with the amount of ethnological information contained in it, especially in the second part, which is exclusively devoted to the aboriginal inhabitants, as stated before; and upon conversing on the subject with some friends, members of the American Ethnological Society, they advised me to translate for publication if not the whole book, at least that part of it which relates to the native population, of which we know, comparatively, perhaps less than of any other portion of the indigenous race of North America. As there is a growing taste for the study of ethnology manifested in this country, and, consequently, a tendency prevailing to collect all materials illustrating the former condition of the American aborigines in different parts of the continent, I complied with the request of my friends, and devoted my hours of leisure to the preparation of this little work, supposing that the account of a man who lived among those Californians a century ago, when their original state had been but little changed by intercourse with Europeans, might be an acceptable addition to our stock of ethnological knowledge.

I have to state, however, that the following pages are not a translation in the strict sense of the word, but a reproduction of the work only as far as it refers to ethnological matters. The reasons which induced me thus to deviate from the usual course of a translator are obvious; for even that portion of the text which treats of the native race contains many things that are not in the least connected with ethnology, the good father being somewhat garrulous and rather fond of moralizing and enlarging upon religious matters, as might be expected from one of his calling; and, although he places the natives of the peninsula exceedingly low in the scale of human development, he takes, nevertheless, occasion to draw comparisons between their barbaric simplicity and the over-refined habits of the Europeans, much in the manner of Tacitus, who seizes upon every opportunity to rebuke the luxury and extravagance of his countrymen, while he describes the rude sylvan life of the ancient inhabitants of Germany. My object being simply to rescue from oblivion a number of facts relating to a portion of the American race, I have omitted all superfluous commentaries indulged in by the author, and, in order to bring kindred subjects under common heads, I have now and then used some freedom in the arrangement of the matter, which is not always properly linked in the original. Although the second part of the book has chiefly furnished the material for this reproduction, I have transferred to the English text, and inserted in the proper places, all those passages in the other divisions, and even in the two appendices that have a bearing upon ethnology, giving thus unity and completeness to the subject, which induced me to prepare these pages. For the rest I have preserved, so far as feasible, the language of the author. Not

much can be said, however, in favor of the style exhibited in the original, and even the spelling of the words defies all rules of orthography, which were adopted a century ago in the German language; nor is our father unaware of his deficiencies, but honestly states in his preface that "if his style was none of the smoothest, and his orthography incorrect in some places, the reader might consider that during the seventeen years of his sojourn in California, comprising the period from 1751 to 1768, he hardly ever had conversed in German, and, consequently, almost forgotten the use of his mother language."

Of the peninsula Father Baegert gives a rather woeful account. He describes that region as an arid, mountainous country, covered with rocks and sand, deficient in water, and almost without shade-trees, but abounding in thorny plants and shrubs of various kinds. The sterility of the soil is caused by the scantiness of water. "No one," says the author, "need be afraid to drown himself in water; but the danger of dying from thirst is much greater." There falls some rain, accompanied by short thunder-storms, during the months of July, August, September, and October, filling the channels worn in the hard ground. Some of these soon become dry after the showers; others, however, hold water during the whole year, and on these and the stagnant water collected in pools and ponds men and beasts have to rely for drink. Of running waters, deserving the name of brooks, there are but six in the country, and of these six only four reach the sea, while the others lose themselves not very far from their sources among rocks and sand. There is nothing to be seen in Lower California that may be called a wood; only a few straggling oaks, pines, and some other kinds of trees unknown in Europe, are met with, and these are confined to certain localities. Shade and material for the carpenter are, therefore, very scarce. The only tree of any consequence is the so-called mesquite; but besides that it always grows quite isolated, and never in groups, the trunk is very low, and the wood so hard that it almost defies the application of iron tools. The author mentions, further, a kind of low Brazil wood, a tree called paloblanco, the bark of which serves for tanning; the palohierro or iron-wood, which is still harder than the mesquite; wild fig trees that bear no fruit; wild willows and barren palms, "all of which would be ashamed to appear beside a European oak or nut-tree." One little tree yields an odoriferous gum that was used in the Californian churches as frankincense. But in compensation for the absence of large trees, there is a prodigious abundance of prickly plants, some of a gigantic height, but of little practical use, their soft, spongy stems soon rotting after being cut. Among the indigenous edible productions of the vegetable kingdom are chiefly mentioned the tunas or Indian figs, the aloë, and the pitahayas, of which the latter deserve a special notice as forming an important article of food of the Indians. There are two kinds of this fruit—the sweet and the sour pitahaya. The former is round, as large as a hen's egg, and has a green, thick, prickly shell that covers a red or white flesh, in which the black seeds are scattered like grains of powder. It is described as being sweet, but not of a very agreeable taste without the addition of lemon juice and sugar. There is no scarcity of shrubs bearing this fruit, and from some it can be gathered by hundreds. They become mature in the middle of June, and continue for more than eight weeks. The sour pitahaya, which grows on low, creeping bushes, bristling with long spines, is much larger than the other kind, of excellent taste, but by far less abundant; for, although the shrubs are very plentiful, there is hardly one among a hundred that bears fruit. Of the aloë or mescale, as the Spaniards and Mexicans call it, the fibres are used by the aborigines, in lieu of hemp, for making threads and strings, and its fruit is eaten by them.

A very curious portion of the book is that which treats of the animals found in California. The author is evidently not much of a naturalist, and, in classifying animals, he manifests occasionally a sovereign independence that would

shock the feelings of a Blumenbach or Agassiz; yet his remarks, resulting from actual observation, are for the most part correct, and evince undeniably his love of truth. In the list of wild quadrupeds are enumerated the deer, hare, rabbit, fox, coyote, wild cat, skunk, (Sorillo,) leopard, (American panther,) onza, and wild ram. In reference to the last-named animal the author remarks: "Where the chain of mountains that runs lengthwise through the whole peninsula reaches a considerable height, there are found animals resembling our rams in all respects, except the horns, which are thicker, longer, and much more curved. When pursued, these animals will drop themselves from the highest precipices upon their horns without receiving any injury. Their number, however, cannot be great, for I never saw a living specimen, nor the fur of one in the possession of an Indian; but many skins of leopards and onzas."

This animal is doubtless identical with the Rocky Mountain sheep, *(Ovis montana.)*

The feathered tribe does not seem to be very plentiful in California, since, according to Father Baegert, a person may travel one or two days without seeing other birds but occasionally a filthy vulture, raven, or "bat." Among the few which he observed are the red-bird, *(cardinal)* blue-bird, humming-bird, and an "ash-colored bird with a tail resembling that of a peacock and a beautiful tuft on its head;" also wild ducks and a species of swallow, the latter appearing only now and then in small numbers, and therefore considered as extraneous.

There are some small fish found in the waters of California; but they do not amount to much, and during lent the father obtained his supply from the Pacific, distant 12 leagues from his habitation. On the other days of abstinence his meal usually consisted of a "little goat-milk and dry beans, and if a few eggs were added, he cared for nothing else, but considered himself well entertained."

Under the comprehensive, but not very scientific head of "vermin," the author enumerates snakes, scorpions, centipedes, huge spiders, toads, wasps, bats, ants, and grasshoppers. These vermin seem to have been a great annoyance to the good missionary, especially the snakes, of which there are about twenty different kinds in California, the rattlesnake being, of course, the most conspicuous among them. This dangerous reptile, which seems to be very numerous in that region, is minutely and correctly described, and, as might be expected, there are also some "snake stories" related. One day when the author was about to shave and took his razors from the upper board of his book-shelf, he discovered there, to his horror, a rattlesnake of large size. He received likewise in his new dwelling-house, which was a stone building, frequent visits from scorpions, large centipedes, tarantulas, ants and toads, all precautions being unavailing against the intrusion of these uninvited guests. The grasshoppers are represented as a real public calamity. Migrating from the southern part of the peninsula towards the north, they deluge the country, obscuring the sun by their numbers, and causing a noise that resembles a strong wind. Never deviating from their line of march, they will climb houses and churches encountered during their progress, laying waste all fields and gardens over which their pernicious train passes.

Of the climate in California the author speaks well, and considers it as both healthy and agreeable. Being only one degree and a half distant from the Tropic of Cancer, he lived, of course, in a hot region, and he remarks with reference to the high temperature that some thought the name "California" was a contraction from the Latin words *calida fornax,* (hot oven,) without vouching, however, for the correctness of the derivation, though he is certain that the appellation is not of Indian origin. The greatest heat begins in the month of July and lasts till the middle of October; but there is every day in the year quite a refreshing wind blowing, which begins at noon, if not sooner, and continues till night. The principal winds are north west and south west; the north

wind blows only now and then during the winter months, but the east wind hardly ever, the latter circumstance being somewhat surprising to the author, who observed that the clouds are almost invariably moving from the east. He never found the cold severer than during the latter part of September or April on the banks of the Rhine, where, after his return, the persevering coldness of winter and clouded atmosphere during that period made him long for the mild temperature and always blue and serene sky of the country he had left. Fogs in the morning are frequent in California, and occur not only during fall and winter, but also sometimes in the hot season. Dew is said to be not more frequent nor heavier than in middle Europe.

Though the author represents California as a dry, sterile country, where but little rain falls, he admits that in those isolated parts where the proximity of water imparts humidity, the soil exhibits an astonishing fertility. "There," he says, "one may plant what he chooses, and it will thrive; there the earth yields fruit a hundred-fold, as in the best countries of Europe, producing wheat and maize, rice, pumpkins, water and other melons of twenty pounds' weight, cotton, lemons, oranges, plantains, pomegranates, excellent sweet grapes, olives and figs, of which the latter can be gathered twice in a summer. The same field yields a double or threefold harvest of maize, that grows to prodigious height, and bears sometimes twelve ears on one stalk. I have seen vines in California that produced in the second year a medium sized basket full of grapes; in the third or fourth year some are as thick as an arm, and shoot forth, in one season, eight and more branches of six feet length. It is only to be regretted that such humid places are of very rare occurrence, and that water for irrigating a certain piece of land sometimes cannot be found within a distance of sixty leagues."

In the last chapter of the first part the author gives an account of the pearl fisheries and silver mines carried on in Lower California while he was there. Both kinds of enterprise are represented as insignificant and by no means very profitable. "Every summer," he says, "eight, ten or twelve poor Spaniards from Sonora, Cinaloa or other parts opposite the peninsula, cross the Gulf in little boats, and encamp on the California shore for the purpose of obtaining pearls. They carry with them a supply of Indian corn and some hundred weight of dried beef, and are accompanied by a number of Mexican Indians, who serve as pearl fishers, for the Californians themselves have hitherto shown no inclination to risk their lives for a few yards of cloth. The pearl fishers are let down into the sea by ropes, being provided with a bag for receiving the pearl oysters which they rake from the rocks and the bottom, and when they can no longer hold their breath, they are pulled up again with their treasure. The oysters, without being opened, are counted, and every fifth one is put aside for the king. Most of them are empty; some contain black, others white pearls, the latter being usually small and ill-shaped. If a Spaniard, after six or eight weeks of hard labor, and after deducting all expenses, has gained a hundred American *pesos* (that is 500 French livres, or a little more than 200 Rhenish florins—a very small sum in America!) he thinks he has made a little fortune which he cannot realize every season. God knows whether the fifth part of the pearls fished in the Californian sea yields, on an average, to the Catholic king 150 or 200 pesos in a year, even if no frauds are committed in the transaction. I heard of only two individuals, with whom I was also personally acquainted, who had accumulated some wealth, after spending twenty and more years in that line of business. The others remained poor wretches, with all their pearl fishing."

There were but two silver mines of any note in operation at the time of Baegert's sojourn in California, and those had been opened only a few years previous to his arrival. They were situated in the districts of St. Anna and St. Antonio, near the southern end of the peninsula, and only three leagues

distant from each other. Digging for silver in California is not represented as a lucrative business, the owner of one of the mines being so poor that he had to beg for his travelling money when he was about to return to Spain. The proprietor of the other mine was in better circumstances, but he owed his wealth more to other speculations than to his subterranean pursuits. The mining population in the two districts amounted to 400 souls, women and children included, and the workmen were either Spaniards born in America, or Indians from the other side of the Californian gulf. The external condition of these people is represented as wretched in the highest degree. The soil produced almost nothing, and not having the necessary money to procure provisions from the Mexican side, they were sometimes compelled to gather their food in the fields, like the native Californians. The author speaks of a locality between the twenty-eighth and twenty-ninth degree, called Rosario, where some supposed gold to exist, but even admitting the fact, he thinks it would be almost impossible to work mines in that region, where neither food for men and beasts, nor water and wood, can be procured. Near the mission of St. Ignatius (28th degree) sulphur is found, and on the islands of El Carmen and St. Joseph in the Californian gulf, and in different places on both coasts salt of very good quality is abundant.

Having thus given an abstract of the first part of the book, I cannot conclude these introductory remarks without saying a few words in favor of the Jesuits. Whatever we may think, as Protestants, of the tendencies of that order, we cannot but admit that those of its members who came as missionaries to America deserve great credit for their zeal in propagating a knowledge of the countries and nations they visited in the New World. To the student of American ethnology particularly, the numerous writings of the Jesuit fathers are of inestimable value, forming, as it were, the very foundations upon which almost all subsequent researches in that interesting field of inquiry are based.

"The missionaries and discoverers whom the order of the Jesuits sent forth were for the most part not only possessed of the courage of martyrs, and of statesmanlike qualities, but likewise of great knowledge and learning. They were enthusiastic travellers, naturalists, and geographers; they were the best mathematicians and astronomers of their time. They have been the first to give us faithful and circumstantial accounts of the new countries and nations they visited. There are few districts in the interior of America concerning which the Jesuits have not supplied us with the oldest and best works, and we can scarcely attempt the study of any American language without meeting with a grammar composed by a Jesuit. In addition to their chapels and colleges in the wilderness, the Jesuits likewise erected observatories; and there are few rivers, lakes, and mountains in the interior, which they have not been the first to draw upon our maps."

With this well-deserved eulogy, which is quoted from Mr. J. G. Kohl's recent work on the discovery of America, I leave to Father Baegert himself the task of relating his experiences among the natives of Lower California.

AN ACCOUNT OF THE ABORIGINAL INHABITANTS OF THE CALIFORNIAN PENINSULA.

CHAPTER I.—THE STATURE, COMPLEXION, AND NUMBER OF THE CALIFORNIANS; ALSO, WHENCE AND HOW THEY MAY HAVE COME TO CALIFORNIA.

In physical appearance the Californians resemble perfectly the Mexicans and other aboriginal inhabitants of America. Their skin is of a dark chestnut or clove color, passing, however, sometimes into different shades, some individuals being of a more swarthy complexion, while others are tan or copper colored.

But in new-born children the color is much paler, so that they hardly can be distinguished from white children when presented for baptism; yet it appears soon after birth, and assumes its dark tinge in a short time. The hair is black as pitch and straight, and seldom turns gray, except sometimes in cases of extreme old age. They are all beardless, and their eye-brows are but scantily provided with hair. The heads of children at their birth, instead of being covered with scales, exhibit hair, sometimes half a finger long. The teeth, though never cleaned, are of the whiteness of ivory. The angles of the eyes towards the nose are not pointed, but arched like a bow. They are well-formed and well-proportioned people, very supple, and can lift up from the ground stones, bones, and similar things with the big and second toes. All walk, with a few exceptions, even to the most advanced age, perfectly straight. Their children stand and walk, before they are a year old, briskly on their feet. Some are tall and of a commanding appearance, others small of stature, as elsewhere, but no corpulent individuals are seen among them, which may be accounted for by their manner of living, for, being compelled to run much around, they have no chance of growing stout.

In a country as poor and sterile as California the number of inhabitants cannot be great, and nearly all would certainly die of hunger in a few days if it were as densely populated as most parts of Europe. There are, consequently, very few Californians, and, in proportion to the extent of the country, almost as few, as if there were none at all; yet, nevertheless, they decrease annually. A person may travel in different parts four and more days without seeing a single human being, and I do not believe that the number of Californians from the promontory of St. Lucas to the Rio Colorado ever amounted, before the arrival of the Spaniards, to more than forty or fifty thousand souls.* It is certain that in 1767, in fifteen, that is, in all the missions, from the 22d to the 31st degree, only twelve thousand have been counted. But an insignificant population and its annual diminution are not peculiar to California alone; both are common to all America. During my journey overland along the east side of the Californian gulf, from Guadalaxara to the river Hiaqui, in the Mexican territory, a distance of four hundred leagues,† I saw only thirteen small Indian villages, and on most days I did not meet a living soul. Father Charlevoix, before setting out on a journey through Canada or New France, writes in his first letter, addressed to the Duchess of Lesdiguières, that he would have to travel sometimes a hundred and more leagues without seeing any human beings besides his companions.‡

With the exception of Mexico and some other countries, North America was, even at the time of the discovery, almost a wilderness when compared with Germany and France; and this is still more the case at the present time. Whoever has read the history of New France by the above-named author, or has travelled six or seven hundred leagues through Mexico, and, besides, obtained reliable information concerning the population of other provinces, can easily form an estimate of the number of native inhabitants in North America; and if the southern half of the New World does not contain a hundred times more inhabitants than the northern part, which, relying on the authority of men who have lived there many years and have travelled much in that country, I am far from believing, those European geographers who speak in their books of 300 millions of Americans are certainly mistaken. Who knows whether they

* Washington Irving states they had numbered from 25,000 to 30,000 souls when the first missions were established; on what authority I do not know.—*Adventures of Captain Bonneville*, (ed. of 1851,) p. 332.

† *Stunden.*—I translate this word by "league," though the French *lieue* is a little longer than the German *stunde.*

‡ Histoire de la Nouvelle France, par le P. de Charlevoix. Paris, 1744; vol. v, p. 66

would find in all more than fifteen or twenty millions? The many hundred languages which are spoken in South America alone are a sure evidence of a scanty population, although the contrary might be inferred at first sight; for, if there were more people, there would be more community among them, the tribes would live closer together, and, as a result, there would be fewer languages. The Ikas in my district speak a language different from that of the other people in my mission; but I am pretty sure that the whole nation of these Ikas never amounted to five hundred persons.

It is easy to comprehend why America is so thinly populated, the manner of living of the inhabitants and their continual wars among themselves being the causes of this deficiency; but how it comes that, since the discovery of the fourth part of the world, its population is constantly melting down, even in those provinces where the inhabitants are not subjected to the Europeans, but retain their full, unrestrained liberty, as, for instance, according to Father Charlevoix, in Louisiana, (that is, in the countries situated on both sides of the Mississippi,) is a question, the solution of which I leave to others, contenting myself with what is written in the Psalms, namely, that the increase or diminution of the human race in different countries is a mystery which man cannot penetrate.

However small the number of Californians is, they are, nevertheless, divided into a great many nations, tribes, and tongues.* If a mission contains only one thousand souls, it may easily embrace as many little nations among its parishioners as Switzerland counts cantons and allies. My mission consisted of Paurus, Atshémes, Mitshirikutamáis, Mitshirikuteurus, Mitshirikutaruanajéres, Teackwàs, Teenguábebes, Utshis, Ikas, Anjukwáres, Utshipujes; all being different tribes, but hardly amounting in all to five hundred souls.

It might be asked, in this place, why there existed fifteen missions on the peninsula, since it appears that 12,000, and even more, Indians could be conveniently superintended and taken care of by three or four priests. The answer is, that this might be feasible in Germany as well as in a hundred places out of Europe, but is utterly impracticable in California; for, if 3 or 4,000 Californians were to live together in a small district, the scanty means of subsistence afforded by that sterile country would soon prove insufficient to maintain them. Besides, all of these petty nations or tribes have their own countries, of which they are as much, and sometimes even more, enamored than other people of theirs, so that they would not consent to be transplanted fifty or more leagues from the place they consider as their home. And, further, the different tribes who live at some distance from each other are always in a mutual state of enmity, which would prevent them from living peaceably together, and offer a serious obstacle to their being enclosed in the same fold. In time of general contagious diseases, lastly, which are of no unfrequent occurrence, a single priest could not perform his duties to their full extent in visiting all his widely scattered patients, and administering to their spiritual and temporal wants. My parish counted far less than a thousand members, yet their encampments were often more than thirty leagues distant from each other. Of the languages and dialects in this country there are also not a few, and a missionary is glad if he has mastered one of them.

It remains now to state my opinion concerning the place where the Californians came from, and in what manner they effected their migration to the country they now occupy. They may have come from different localities, and either voluntarily or by some accident, or compelled by necessity; but that people

* The author probably fell into the very common error of confounding dialects with languages. Dr. Waitz, relying on Buschmann's linguistic researches, mentions only three *principal* languages spoken by the natives of Lower California, viz., the Pericu, Monqui, and Cochimi languages.—*Anthropologie der Naturvölker von Dr. Theodor Waitz.* Leipzig, 1864; vol iv, p. 248.

should have migrated to California of their own free will, and without compulsion, I am unable to believe. America is very large, and could easily support fifty times its number of inhabitants on much better soil than that of California. How, then, is it credible that men should have pitched, from free choice, their tents amidst the inhospitable dreariness of these barren rocks? It is not impossible that the first inhabitants may have found by accident their way across the sea from the other side of the Californian gulf, where the provinces of Cinaloa and Sonora are situated; but, to my knowledge, navigation never has been practiced by the Indians of that coast, nor is it in use among them at the present time. There is, furthermore, within many leagues towards the interior of the country no kind of wood to be had suitable for the construction of even the smallest vessel. From the Pimeria, the northernmost country opposite the peninsula, a transition might have been easier either by land, after crossing the Rio Colorado, or by water, the sea being in this place very narrow and full of islands. In default of boats they could employ their balsas or little rafts made of reeds, which are also used by my Californians who live near the sea, either for catching fish or turtle, or crossing over to a certain island distant two leagues from the shore. I am, however, of opinion that, if these Pimerians ever had gone to California induced by curiosity, or had been driven to that coast by a storm, the dreary aspect of the country soon would have caused them to return without delay to their own country. It was doubtless necessity that gave the impulse to the peopling of the peninsula. Nearly all neighboring tribes of America, over whom the Europeans have no sway, are almost without cessation at war with each other, as long as one party is capable of resistance; but when the weaker is too much exhausted to carry on the feud, the vanquished usually leaves the country and settles in some other part at a sufficient distance from its foes. I am, therefore, inclined to believe that the first inhabitants, while pursued by their enemies, entered the peninsula by land from the north side, and having found there a safe retreat they remained and spread themselves out. If they had any traditions, some light might be thrown on this subject; but no Californian is acquainted with the events that occurred in the country prior to his birth, nor does he even know who his parents were if he should happen to have lost them during his infancy.

To all appearance the Californians, at least those toward the south, believed, before the arrival of the Spaniards in their country, that California constituted the whole world, and they themselves its sole inhabitants; for they went to nobody, and nobody came to see them, each little people remaining within the limits of its small district. Some of those under my care believed to be derived from a bird; some traced their origin from a rock that was lying not far from my house; while others ascribed their descent to still different, but always equally foolish and absurd sources.

CHAPTER II.—THEIR HABITATIONS, APPAREL, IMPLEMENTS, AND UTENSILS.

With the exception of the churches and dwellings of the missionaries, which every one, as well as he could, and as time and circumstances permitted, built of stone and lime, of stone and mud, of huge unburnt bricks, or other materials, and besides some barracks which the Indians attached to the missions, the few soldiers, boatmen, cowherds, and miners have now erected in the fourteen stations, nothing is to be seen in California that bears a resemblance to a city, a village, a human dwelling, a hut, or even a dog-house. The Californians themselves spend their whole life, day and night, in the open air, the sky above them forming their roof, and the hard soil the couch on which they sleep. During winter, only, when the wind blows sharp, they construct around them, but only opposite the direction of the wind, a half moon of brush-wood, a few spans high,

as a protection against the inclemency of the weather,* showing thus that, notwithstanding their simplicity, they understand pretty well "how to turn the mantle towards the wind."† It cannot be otherwise with them; for, if they had houses, they would be compelled to carry their dwellings always with them, like snails or turtles, the necessity of collecting food urging them to wander constantly about. Thus they cannot start every morning from the same place and return thither in the evening, since, notwithstanding the small number of each little people, a small tract of land could not provide them with provisions during a whole year. To-day the water will fail them; to-morrow they have to go to some locality for gathering a certain kind of seed that serves them as food, and so they fulfil to the letter what is written of all of us, namely, that we shall have no fixed abode in this world. I am certainly not much mistaken in saying that many of them change their night-quarters more than a hundred times in a year, and hardly sleep three times successively in the same place and the same part of the country, always excepting those who are connected with the missions. Wherever the night surprises them they will lie down to sleep, not minding in the least the uncleanliness of the ground, or apprehending any inconvenience from reptiles and other vermin, of which there is an abundance in this country. They do not live under the shade of trees, as some authors have said, because there are hardly any trees in California that afford shade, nor do they dwell in earth-holes of their own making, as others have said, but sometimes, and only when it rains, they resort to the clefts and cavities of rocks, if they can find such sheltering places, which do not occur as frequently as their wants require.

Whenever they undertake to construct shelters for protecting their sick from heat or cold, the entrance is usually so low that a person has to creep on hands and feet in order to get in, and the whole structure is of such small dimensions as to render it impossible to stand erect within, or to find room to sit down on the ground for the purpose of confessing or comforting the patient. Of no better condition are the huts of those Indians who live near the missions, the same being often so small and miserable that man and wife hardly can sit or lie down in them. Even the old and infirm are utterly indifferent as to their being under shelter or not, and it happened often that I found old sick persons lying in the open air, for whose accommodation I had caused huts to be built on the preceding day. So much for habit.

As the blue sky forms the only habitation of the Californian Indians, so they wear no other covering than the brown skin with which nature has clothed them. This applies to the male sex in the full sense of the word, and even women have been found in the northern parts of California in a perfect state of nudity, while among most nations the females always covered themselves to a small extent. They did, and still continue to do, as follows: They understand how to prepare from the fibres of the aloë plant a white thread, which serves them for making cords.‡ On these they string hundreds of small sections of water-reed, like beads of a rosary; and a good number of these strings, attached by their ends to a girdle, and placed very close and thick together, form two aprons, one of which hangs down below the abdomen, while the other covers the hind part. These aprons are about a span wide, and of different length. Among

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* Captain Bonneville gives a cheerless account of a village of the Root Diggers, which he saw in crossing the plain below Powder river. "They live," says he, "without any further protection from the inclemency of the season than a sort of break-weather, about three feet high, composed of sage, (or wormwood,) and erected around them in the shape of a half moon."—*Washington Irving: Adventures of Captain Bonneville*, p. 259.

† German proverb.

‡ It may not be out of place to mention here that in Mexico the dried fibres of the aloë or maguey plant (*Agave Americana*) are a universal substitute for hemp in the manufacture of cordage and packing-cloth.

some nations they reach down to the knees; among others to the calves, and even to the feet. Both sides of the thighs, as well as the rest of the body, remain perfectly naked. In order to save labor, some women wear, instead of the back-aprons, a piece of untanned deer-skin, or any woollen or linen rag which they can now-a-days obtain. Of the same untanned skin they make, if they can get it, their shoes or sandals, simply flat pieces, which they attach to the feet by coarse strings of the above-mentioned aloë, passing between the big and small toes and around the ankles.

Both sexes, the grown as well as the children, wear the head always uncovered, however inclement the weather may be, even those in a certain mission who understand how to manufacture pretty good hats from palm-leaves, which, on account of their lightness, were frequently worn by the missionaries while on their travels. The men allow the hair to grow down to the shoulders. Women, on the contrary, wear it much shorter. Formerly they pierced the ears of new-born children of the male sex with a pointed stick, and by putting bones and pieces of wood into the aperture they enlarged it to such a degree that, in some grown persons, the flaps hung down nearly to the shoulders. At present, however, they have abandoned this unnatural usage. It has been asserted that they also pierce the nose. I can only say that I saw no one disfigured in that particular manner, but many middle-aged persons with their ears perforated as described above. Under certain circumstances, and on their gala days, they paint different parts of the body with red and yellow color, which they obtain by burning certain minerals.

The baptized Indians, of course, observed more decency in regard to dress. The missionaries gave each male individual, once or twice in a year, a piece of blue cloth, six spans long and two spans wide, for covering the lower part of the body, and, if their means allowed it, a short woollen coat of blue color. The women and girls were provided with thick white veils, made of wool, that covered the head and the whole body down to the feet. In some missions the women received also petticoats and jackets of blue flannel or woven cotton shirts, and the men trowsers of coarse cloth and long coats. But the women throw aside their veils, and the men their coats, as soon as they leave church, because those coverings make them feel uneasy, especially in summer, and impede the free use of their limbs, which their mode of living constantly requires. I will mention here that all these goods had to be brought from the city of Mexico, since nothing of the kind can be manufactured in California for want of the necessary materials. The number of sheep that can be kept there is small, and, moreover, they lose half their wool by passing through the thorny shrubs, of which there is an astonishing abundance in this ill-favored country.

It is not to be expected that a people in as low a state of development as the Californians should make use of many implements and utensils. Their whole furniture, if that expression can be applied at all, consists of a bow and arrows, a flint instead of a knife, a bone or pointed piece of wood for digging roots, a turtle-shell serving as basket and cradle, a large gut or bladder for fetching water and transporting it during their excursions, and a bag made like a fishing net from the fibres of the aloë, or the skin of a wild cat, in which they preserve and carry their provisions, sandals, and perhaps other insignificant things which they may happen to possess.

The bows of the Californians are more than six feet long, slightly curved, and made from the roots of wild willows. They are of the thickness of the five fingers in the middle, round, and become gradually thinner and pointed towards the ends. The bow-strings are made of the intestines of beasts. The shafts of their arrows consist of common reeds, which they straighten by the fire. They are above six spans long, and have, at the lower end, a notch to catch the string, and three or four feathers, about a finger long, not much projecting, and let into slits made for that purpose. At the upper end of the shaft

a pointed piece of heavy wood, a span and a half long, is inserted, bearing usually at its extremity a flint of a triangular shape, almost resembling a serpent's tongue, and indented like the edge of a saw.* The Californians carry their bows and arrows always with them, and as they commence at an early age to use these weapons many of them become very skilful archers.

In lieu of knives and scissors they use sharp flints for cutting almost everything—cane, wood, aloë, and even their hair—and for disembowelling and skinning animals. With the same flints they bleed or scarify themselves, and make incisions for extracting thorns and splinters which they have accidentally run into their limbs.

The whole art of the men consists in the manufacture of bows and arrows, while the mechanical skill of the females is merely confined to the making of the above-mentioned aprons. Of a division of labor not a trace is to be found among them; even the cooking is done by all without distinction of sex or age, every one providing for himself, and the children commence to practice that necessary art as soon as they are able to stir a fire. The time of these people is chiefly taken up by the search for food and its preparation; and if their physical wants are supplied they abandon themselves entirely to lounging, chattering, and sleep. This applies particularly to the roaming portion of the Californian Indians, for those who dwell near the missions now established in the country are sometimes put to such labor as the occasion may require.

CHAPTER III.—OF THEIR FOOD AND THE MANNER OF PREPARING IT.

Notwithstanding the barrenness of the country, a Californian hardly ever dies of hunger, except, perhaps, now and then an individual that falls sick in the wilderness and at a great distance from the mission, for those who are in good health trouble themselves very little about such patients, even if these should happen to be their husbands, wives, or other relations; and a little child that has lost its mother or both parents is also occasionally in danger of starving to death, because in some instances no one will take charge of it, the father being sometimes inhuman enough to abandon his offspring to its fate.

The food of the Californians, as will be seen, is certainly of a mean quality, yet it keeps them in a healthy condition, and they become strong and grow old in spite of their poor diet. The only period of the year during which the Californians can satisfy their appetite without restraint is the season of the pitahayas, which ripen in the middle of June and abound for more than eight weeks. The gathering of this fruit may be considered as the harvest of the native inhabitants. They can eat as much of it as they please, and with some this food agrees so well that they become corpulent during that period; and for this reason I was sometimes unable to recognize at first sight individuals, otherwise perfectly familiar to me, who visited me after having fed for three or four weeks on these pitahayas. They do not, however, preserve them, and when the season is over they are put again on short rations. Among the roots eaten by the Californians may be mentioned the yuka, which constitutes an important article of food in many parts of America, as, for instance, in the island of Cuba, but is not very abundant in California. In some provinces it is made into a kind of bread or cake, while the Californians, who would find this process too tedious, simply roast the yukas in a fire like potatoes. Another root eaten by the natives is that of the aloë plant, of which there are many kinds in this country. Those species of this vegetable, however, which afford nourishment —for not all of them are edible—do not grow as plentifully as the Californians might wish, and very seldom in the neighborhood of water; the prepara-

* In the collection of Dr. E. H. Davis, of New York, there are a number of arrows obtained from the Indians of the island of Tiburon, in the Californian gulf. They answer, in every respect, the description given in the text.

tions, moreover, which are necessary to render this plant eatable, require much time and labor, as will be mentioned hereafter. I saw the natives also frequently eat the roots of the common reed, just as they were taken out of the water. Certain seeds, some of them not larger than those of the mustard, and different sorts in pods that grow on shrubs and little trees, and of which there are, according to Father Piccolo, more than sixteen kinds, are likewise diligently sought; yet they furnish only a small quantity of grain, and all that a person can collect with much toil during a whole year may scarcely amount to twelve bushels.*

It can be said that the Californians eat, without exception, all animals they can obtain. Besides the different kinds of larger indigenous quadrupeds and birds already mentioned,† they live now-a-days on dogs and cats; horses, asses and mules; *item*, on owls, mice and rats; lizards and snakes; bats, grasshoppers and crickets; a kind of green caterpillar without hair, about a finger long, and an abominable white worm of the length and thickness of the thumb, which they find occasionally in old rotten wood, and consider as a particular delicacy. The chase of game, such as deer and rabbits, furnishes only a small portion of a Californian's provisions. Supposing that for a hundred families three hundred deer are killed in the course of a year, which is a very favorable estimate, they would supply each family only with three meals in three hundred and sixty-five days, and thus relieve but in a very small degree the hunger and the poverty of these people. The hunting for snakes, lizards, mice and field-rats, which they practice with great diligence, is by far more profitable and supplies them with a much greater quantity of articles for consumption. Snakes, especially, are a favorite sort of small game, and thousands of them find annually their way into the stomachs of the Californians.

In catching fish, particularly in the Pacific, which is much richer in that respect than the gulf of California, the natives use neither nets‡ nor hooks, but a kind of lance,—that is, a long, slender, pointed piece of hard wood, which they handle very dexterously in spearing and killing their prey. Sea-turtles are caught in the same manner.

I have now mentioned the different articles forming the ordinary food of the Californians; but, besides these, they reject nothing that their teeth can chew or their stomachs are capable of digesting, however tasteless or unclean and disgusting it may be. Thus they will eat the leaves of the Indian fig-tree, the tender shoots of certain shrubs, tanned or untanned leather; old straps of raw hide with which a fence was tied together for years; *item*, the bones of poultry, sheep, goats and calves; putrid meat or fish swarming with worms, damaged wheat or Indian corn, and many other things of that sort which may serve to appease the hunger they are almost constantly suffering. Anything that is thrown to the hogs will be also accepted by a Californian, and he takes it without feeling offended, or thinking for a moment that he is treated below his dignity. For this reason no one took the trouble to clean the wheat or maize, which was cooked for them in a large kettle, of the black worms and little bugs, even if the numbers of these vermin had been equal to that of the grains. By a daily distribution of about 150 bushels of bran, (which they are in the habit of eating without any preparation,) I could have induced all my parishioners

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* One *malter*, in German, which is about equivalent to twelve bushels.

† In the introduction.

‡ Venegas mentions fishing-nets made of the *pita* plant, (Noticia de la California, vol. i, p. 52.) According to Baegert, (Appendix i, p. 322,) no such plant exists in California, and the word "pita" only signifies the thread twisted from the aloë. In refuting Venegas, Father Baegert hardly ever refers to the original Spanish work, nor mentions the name of its author, but attacks the French translation, which was published in Paris in the year 1767. He probably acted so from motives of delicacy, Venegas himself being a priest and brother Jesuit. The effect of this proceeding, as can be imagined, is comical in a high degree.

to remain permanently in the mission, excepting during the time when the pitahayas are gathered.

I saw one day a blind man, seventy years of age, who was busily engaged in pounding between two stones an old shoe made of raw deer-skin, and whenever he had detached a piece, he transferred it promptly to his mouth and swallowed it; and yet this man had a daughter and grown grand-children. As soon as any of the cattle are killed and the hide is spread out on the ground to dry, half a dozen boys or men will instantly rush upon it and commence to work with knives, flints and their teeth, tearing and scratching off pieces, which they eat immediately, till the hide is full of holes or scattered in all directions. In the mission of St. Ignatius and in others further towards the north, there are persons who will attach a piece of meat to a string and swallow it and pull it out again a dozen times in succession, for the sake of protracting the enjoyment of its taste.

I must here ask permission of the kind reader to mention something of an exceedingly disgusting and almost inhuman nature, the like of which probably never has been recorded of any people in the world, but which demonstrates better than anything else the whole extent of the poverty, uncleanness and voracity of these wretched beings. In describing the pitahayas,* I have already stated that they contain a great many small seeds resembling grains of powder. For some reason unknown to me these seeds are not consumed in the stomach, but pass off in an undigested state, and in order to save them the natives collect, during the season of the pitahayas, that which is discharged from the human body, separate the seeds from it, and roast, grind and eat them, making merry over their loathsome meals, which the Spaniards therefore call the second harvest of the Californians.† When I first heard that such a filthy habit existed among them, I was disinclined to believe the report, but to my utter regret I became afterwards repeatedly a witness to the proceeding, which they are unwilling to abandon like many other bad practices. Yet I must say in their favor that they have always abstained from human flesh, contrary to the horrible usage of so many other American nations who can obtain their daily food much easier than these poor Californians.

They have no other drink but the water, and Heaven be praised that they are unacquainted with such strong beverages as are distilled in many American provinces from Indian corn, the aloë and other plants, and which the Americans in those parts merely drink for the purpose of intoxicating themselves. When a Californian encounters, during his wanderings, a pond or pool, and feels a desire to quench his thirst, he lies flat on the ground and applies his mouth directly to the water. Sometimes the horns of cattle are used as drinking vessels.

Having thus far given an account of the different articles used as aliment by the aborigines of the peninsula, I will now proceed to describe in what manner they prepare their victuals. They do not cook, boil, or roast like people in civilized countries, because they are neither acquainted with these methods, nor possessed of vessels and utensils to employ for such purposes; and, besides, their patience would be taxed beyond endurance, if they had to wait till a piece of meat is well cooked or thoroughly roasted. Their whole process simply consists in burning, singeing, or roasting in an open fire all such victuals as are not eaten in a raw state: Without any formalities the piece of meat, the fish, bird, snake, field-mouse, bat, or whatever it may be, is thrown into the flames, or on the glowing embers, and left there to smoke and to sweat for about a quarter of an hour; after which the article is withdrawn, in most cases

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* Introduction.

† This statement is corroborated in all particulars by Clavigero, in his *Storia della California*, (Venice, 1789,) vol. i, p. 117.

only burned or charred on the outside, but still raw and bloody within. As soon as it has become sufficiently cool, they shake it a little in order to remove the adhering dust or sand, and eat it with great relish. Yet I must add here, that they do not previously take the trouble to skin the mice or disembowel the rats, nor deem it necessary to clean the half-emptied entrails and maws of larger animals, which they have to cut in pieces before they can roast them. Seeds, kernels, grasshoppers, green caterpillars, the white worms already mentioned, and similar things that would be lost, on account of their smallness, in the embers and flames of an open fire, are parched on hot coals, which they constantly throw up and shake in a turtle-shell, or a kind of frying-pan woven out of a certain plant. What they have parched or roasted in this manner is ground to powder between two stones, and eaten in a dry state. Bones are treated in like manner.

They eat everything unsalted, though they might obtain plenty of salt; but since they cannot dine every day on roast meat and constantly change their quarters, they would find it too cumbersome to carry always a supply of salt with them.

The preparation of the aloë, also called *mescale* or *maguey* by the Spaniards, requires more time and labor. The roots, after being properly separated from the plants, are roasted for some hours in a strong fire, and then buried, twelve or twenty together, in the ground, and well covered with hot stones, hot ashes, and earth. In this state they have to remain for twelve or fourteen hours, and when dug out again they are of a fine yellow color, and perfectly tender, making a very palatable dish, which has served me frequently as food when I had nothing else to eat, or as dessert after dinner in lieu of fruit. But they act at first as a purgative on persons who are not accustomed to them, and leave the throat somewhat rough for a few hours afterwards.

To light a fire the Californians make no use of steel and flint, but obtain it by the friction of two pieces of wood. One of them is cylindrical, and pointed on one end, which fits into a round cavity in the other, and by turning the cylindrical piece with great rapidity between their hands, like a twirling stick, they succeed in igniting the lower piece, if they continue the process for a sufficient length of time.

The Californians have no fixed time for any sort of business, and eat, consequently, whenever they have anything, or feel inclined to do so, which is nearly always the case. I never asked one of them whether he was hungry, who failed to answer in the affirmative, even if his appearance indicated the contrary. A meal in the middle of the day is the least in use among them, because they all set out early in the morning for their foraging expeditions, and return only in the evening to the place from which they started, if they do not choose some other locality for their night quarters. The day being thus spent in running about and searching for food, they have no time left for preparing a dinner at noon. They start always empty-handed; for, if perchance something remains from their evening repasts, they certainly eat it during the night in waking moments, or on the following morning before leaving. The Californians can endure hunger easier and much longer than other people; whereas they will eat enormously if a chance is given. I often tried to buy a piece of venison from them when the skin had but lately been stripped off the deer, but regularly received the answer that nothing was left; and I knew well enough that the hunter who killed the animal needed no assistance to finish it. Twenty-four pounds of meat in twenty-four hours is not deemed an extraordinary ration for a single person, and to see anything eatable before him is a temptation for a Californian which he cannot resist; and not to make away with it before night would be a victory he is very seldom capable of gaining over himself.

One of them requested from his missionary a number of goats, in order to live, as he said, like a decent man; that is, to keep house, to pasture the goats, and to support himself and his family with their milk and the flesh of the kids. But, alas! in a few days the twelve goats with which the missionary had presented him were all consumed.

A priest who had lived more than thirty years in California, and whose veracity was beyond any doubt, assured me repeatedly that he had known a Californian who one day ate seventeen watermelons at one sitting; and another native who, after having received from a soldier six pounds of unclarified sugar as pay for a certain debt, sat down and munched one piece after another till the six pounds had disappeared. He paid, however, dearly for his gluttony, for he died in consequence of it; while the melon-eater was only saved by taking a certain physic which counteracted the bad effects of his greediness. I was called myself one evening in great haste to three or four persons, who pretended to be dying, and wanted to confess. These people belonged to a band of about sixty souls, (women and children included,) to whom I had given, early in the morning, three bullocks in compensation for some labor. When I arrived at the place where they lay encamped, I learned that their malady consisted merely in belly-ache and vomiting; and, recognizing at once the cause of their disorder, I reprimanded them severely for their voracity, and went home again.

CHAPTER IV.—OF THEIR MARRIAGES AND THE EDUCATION OF THEIR CHILDREN.

As soon as the young Californian finds a partner, the marriage follows immediately afterwards; and the girls go sometimes so far as to demand impetuously a husband from the missionary, even before they are twelve years old, which is their legitimate age for marrying. In all the missions, however, only one excepted, the number of men was considerably greater than that of the females.

Matrimonial engagements are concluded without much forethought or scruple, and little attention is paid to the morals or qualities of the parties; and, to confess the truth, there is hardly any difference among them in these respects; and, as far as good sense, virtue, and riches are concerned, they are always sure to marry their equals, following thus the old maxim: *Si vis nubere, nube pari.* It happens very often that near relations want to join in wedlock, and their engagements have, therefore, to be frustrated, such cases excepted in which the *impedimentum affinitatis* can be removed by a dispensation from the proper authorities.

They do not seem to marry exactly for the same reasons that induce civilized people to enter into that state; they simply want to have a partner, and the husband, besides, a servant whom he can command, although his authority in that respect is rather limited, for the women are somewhat independent, and not much inclined to obey their lords. Although they are now duly married according to the rites of the Catholic church, nothing is done on their part to solemnize the act; none of the parents or other relations and friends are present, and no wedding feast is served up, unless the missionary, instead of receiving his marriage fees, or *jura stolae*, presents them with a piece of meat, or a quantity of Indian corn. Whenever I joined a couple in matrimony, it took considerable time before the bridegroom succeeded in putting the wedding ring on the right finger of his future wife. As soon as the ceremony is over, the new married couple start off in different directions in search of food, just as if they were not more to each other to-day than they were yesterday; and in the same manner they act in future, providing separately for their support, sometimes without living together for weeks, and without knowing anything of their partner's abiding place.

Before they were baptized each man took as many wives as he liked, and if there were several sisters in a family he married them all together. The son-in-law was not allowed, for some time, to look into the face of his mother-in-law or his wife's next female relations, but had to step aside, or to hide himself, when these women were present. Yet they did not pay much attention to consanguinity, and only a few years since one of them counted his own daughter (as he believed) among the number of his wives. They met without any formalities, and their vocabulary did not even contain the words "to marry," which is expressed at the present day in the Waïcuri language by the paraphrase *tikére undiri*—that is, "to bring the arms or hands together." They had, and still use, a substitute for the word "husband," but the etymological meaning of that expression implies an intercourse with women in general.

They lived, in fact, before the establishment of the missions in their country, in utter licentiousness, and adultery was daily committed by every one without shame and without any fear, the feeling of jealousy being unknown to them. Neighboring tribes visited each other very often only for the purpose of spending some days in open debauchery, and during such times a general prostitution prevailed. Would to God that the admonitions and instructions of those who converted these people to Christianity and established lawful marriages among them, had also induced them to desist entirely from these evil practices! Yet they deserve pity rather than contempt, for their manner of living together engenders vice, and their sense of morality is not strong enough to prevent them from yielding to the temptations to which they are constantly exposed.

In the first chapter of this book I have already spoken of the scanty population of this country. It is certain that many of their women are barren, and that a great number of them bear not more than one child. Only a few out of one or two hundred bring forth eight or ten times, and if such is really the case, it happens very seldom that one or two of the children arrive at a mature age. I baptized, in succession, seven children of a young woman, yet I had to bury them all before one of them had reached its third year, and when I was about to leave the country I recommended to the woman to dig a grave for the eighth child, with which she was pregnant at the time. The unmarried people of both sexes and the children generally make a smaller group than the married and widowed.

The Californian women lie in without difficulty, and without needing any assistance. If the child is born at some distance from the mission they carry it thither themselves on the same day, in order to have it baptized, not minding a walk of two or more leagues. Yet, that many infants die among them is not surprising; on the contrary, it would be a wonder if a great number remained alive. For, when the poor child first sees the light of day, there is no other cradle provided for it but the hard soil, or the still harder shell of a turtle, in which the mother places it, without much covering, and drags it about wherever she goes. And in order to be unencumbered, and enabled to use her limbs with greater freedom while running in the fields, she will leave it sometimes in charge of some old woman, and thus deprive the poor creature for ten or more hours of its natural nourishment. As soon as the child is a few months old the mother places it, perfectly naked, astraddle on her shoulders, its legs hanging down on both sides in front, and it has consequently to learn how to ride before it can stand on its feet. In this guise the mother roves about all day, exposing her helpless charge to the hot rays of the sun and the chilly winds that sweep over the inhospitable country. The food of the child, till it cuts its teeth, consists only in the milk of the mother, and if that is wanting or insufficient, there is rarely another woman to be found that would be willing, or, perhaps, in the proper condition, to take pity on the poor starving being. I cannot say that the Californian women are too fond of their children, and some of them may even consider the loss of one as a relief from a burden, especially if they have

already some small children. I did not see many Californian mothers who caressed their children much while they lived, or tore their hair when they died, although a kind of dry weeping is not wanting on such occasions. The father is still more insensible, and does not even look at his (or at least his wife's) child as long as it is small and helpless.

Nothing causes the Californians less trouble and care than the education of their children, which is merely confined to a short period, and ceases as soon as the latter are capable of making a living for themselves—that is, to catch mice and to kill snakes. If the young Californians have once acquired sufficient skill and strength to follow these pursuits, it is all the same to them whether they have parents or not. Nothing is done by these in the way of admonition or instruction, nor do they set an example worthy to be imitated by their offspring. The children do what they please, without fearing reprimand or punishment, however disorderly and wicked their conduct may be. It would be well if the parents did not grow angry when their children are now and then slightly chastised for gross misdemeanor by order of the missionary; but, instead of bearing with patience such wholesome correction of their little sons and daughters, they take great offence and become enraged, especially the mothers, who will scream like furies, tear out the hair, beat their naked breasts with a stone, and lacerate their heads with a piece of wood or bone till the blood flows, as I have frequently witnessed on such occasions.*

The consequence is, that the children follow their own inclinations without any restraint, and imitate all the bad habits and practices of their equals, or still older persons, without the slightest apprehension of being blamed by their fathers and mothers, even if these should happen to detect them in the act of committing the most disgraceful deeds. The young Californians who live in the missions commence roaming about as soon as mass is over, and those that spend their time in the fields go wherever, and with whomsoever, they please, not seeing for many days the faces of their parents, who, in their turn, do not manifest the slightest concern about their children, nor make any inquiries after them. These are disadvantages which the missionary has no power of amending, and such being the case, it is easy to imagine how little he can do by instruction, exhortation, and punishment, towards improving the moral condition of these young natives.

Heaven may enlighten the Californians, and preserve Europe, and especially Germany, from such a system of education, which coincides, in part, with the plan proposed by that ungodly visionary, J. J. Rousseau, in his "Emile," and which is also recommended by some other modern philosophers of the same tribe. If their designs are carried out, education, so far as faith, religion, and the fear of God are concerned, is not to be commenced before the eighteenth or twentieth year, which, if viewed in the proper light, simply means to adopt the Californian method, and to bring up youth without any education at all.

(TO BE CONTINUED IN THE NEXT REPORT.)

* This statement does not seem to agree well with the alleged indifference of the Californian women towards their children, and the formalities which the Californians were obliged to observe, when meeting with the mothers and other female relations of their wives, renders a total absence of jealousy among them rather doubtful. Dr. Waitz has also pointed out the latter discrepancy while citing a number of facts contained in our author's work, (Anthropologie der Naturvœlker, vol. iv, p. 250.) My object being simply to give an English version of Baegert's account, I abstain from all comments on such real or seeming incongruities.

ETHNOLOGY.

FROM THE LONDON ATHÆNEUM.

HALIFAX, NOVA SCOTIA, *June* 21. 1863.

DURING the last winter's session of the Nova Scotian Institute of Natural Science, the Rev. John Ambrose, rector of the parish of St. Margaret's bay, a district lying on the Atlantic seaboard of this colony, brought to the notice of the Institute the existence of extensive beds of refuse shells and bones, mixed with fragments of rude pottery, and perfect and imperfect flint arrow and spear heads. Gifted with an inquiring mind, the gentleman in question naturally considered that their occurrence was not a matter of chance; and, following up the subject, he ascertained that similar beds had been known to exist on the shores of Denmark and the adjacent isles, and that they had received the name of *kjækken-mædding*, or kitchen-middings, from being heaps of refuse shells, bones, &c., thrown aside by the primitive race of men who, in days of remote antiquity, visited annually, or dwelt continuously, in such positions. On perusing an article published in the report of the Smithsonian Institution for 1860, which gave an interesting account of the kitchen middings of Europe, as surveyed by the Danish archeologists, a perfect resemblance to those of the Nova Scotian coast was at once perceived, in so far at least as the few specimens then obtained from these heaps proved.

To endeavor to make a thorough search, and prove the nature of these deposits, the Council of the Institute of Natural Science decided upon having a field meeting on the spot where the kitchen middings lay; and, accordingly, on the 11th of June last, a large party proceeded by land from Halifax, the capital of the province, to St. Margaret's bay, which is distant, in a S.SW. direction, about twenty-two miles. This bay is exceedingly spacious, runs inland some eight or ten miles, and is in breadth, perhaps, five or six miles. A few islands stand at the entrance as well as at its head, and long, low, promontories, clothed with spruce, birch, and maple, stretch into the water at the NE. corner, forming snug coves and sheltered strands. It is on the shore of one of these minor bays, having a sandy beach, where canoes could be hauled up easily and safely, that the principal *kjækken-mædding*, found by Mr. Ambrose, lay, on a rising knoll some twenty feet above the bay at high-water mark. It forms part of a grass field belonging to a farm-house hard by, and according to the statement of the farmer, and the appearance it presents, has been submitted to little, if any, disturbance at the hand of man. The deposit appears to have extended about fifty yards or more in length by a well-defined breadth of eight yards. Its surface is irregularly depressed and dotted over, on its western extremity, with granitic boulders of no great size. The soil which covers the mass is similar to that of the field in which it occurs, though, perhaps, a little darker in color. It grows common meadow grass and the ordinary field plants, and its depth does not exceed two or three inches when the shell deposit appears, presenting a layer of compact shells, perfect and imperfect, in which lie bones of animals and birds, flint and quartz arrow and spear heads, large and small teeth, and broken pieces of very roughly made pottery, bearing evident traces of attempt at ornament. This pottery was very dark in color, and contained in its substance grains of granitic sand, and mica in quantity. From the pieces of rim obtained, judging from their curvature, the earthen vessels could scarcely have exceeded the dimensions of a quart bowl. These bowls or cups must have been in common use, as the fragments occur in some plenty. No traces

of implements denoting any connexion with the later iron age occurred, and the only objects on which the art of man had been practiced beyond the pottery and flint weapon-heads were bones sharpened into awls, one of which was obtained in a very perfect state.

In the midst, but more abundantly at the bottom, of the refuse deposits occurred rounded stones, from the size of a man's clenched hand and upwards, bearing evident traces of having undergone the action of fire. These stones are precisely similar to those found on the beach beneath.

At the bottom of the refuse heap, which occurred at a distance of eighteen inches from the surface, a layer of black soil came two inches thick; then a layer of white brown sand of the same thickness; then came a reddish colored earth, getting lighter as the spade went down, until the original foundation of hardened drift proclaimed no further investigation necessary in that direction. Taking a general view of the surface, the observer naturally supposed that the rounded granitic boulders which lie scattered on the heap had afforded seats for the primitive people, who rudely cooked their food at this encampment on the edge of the wild forest; nor was the supposition incorrect, for on digging around these boulders greater masses of shells, and more evident traces of fire were apparent than in other parts of the heap. The charcoal, in some instances, had lost but little of its former consistency, while in others it powdered into dust on being handled. This probably arose from the nature of the wood, some kinds affording a hard charcoal, and others soft.

The Fauna of this Nova Scotian *kjækken-mædding*, so far as it could be ascertained, was as follows: Of mammals, the moose, *(Cervus alces,)* the bear, *(Ursus americanus,)* the beaver, *(Castor canadensis,)* and the porcupine, *(Hystrix dorsata,)* were noticed; the beaver and porcupine by their teeth, which, from their brightness and compactness, might just have been taken from the jaw. A beaver's tooth had the root part rubbed, and smoothed to a head, giving, with its chisel-like point, the appearance of an instrument for cutting. Some of these teeth were jagged on their edges as if by artificial means. The bones of the animals had been broken, and, with the exception of a few very small ones, none were obtained whole. Of birds, there were the bones of different species, some very large, and evidently belonging to a bird much larger than the great northern diver, *(Colymbus glacialis,)* which is one of the largest wild birds in the colony at the present day. The bird bones were also more or less broken, and one in particular had been opened by means of a cutting instrument down the side. Of fishes, the vertebræ of two or three species, the largest measuring about an inch in diameter, while two or three specimens of the opercular spines of the Norway haddock, *(Sebastes norwegians,)* were procured among the debris in a perfect state, which led to the supposition that they were used for some purpose, such as pricking holes. Of mollusks, the most common were the quahog, *(Venus mercenaria,)* clam, *(Mya arenaria,)* scallop, *(Pecten islandicus,) Crepidula fornicata* and *Mytilus edulis.* Of the two former species nearly the whole mass of shell consisted. The mussel shells had become so friable that the slightest touch was sufficient to break them.

Time did not permit, however, a closer examination to be made on this first visit to the mounds; but some members of the Institute, aware of the interest attaching to the subject, have decided upon camping out during the ensuing summer in the vicinity of other deposits known to exist in various places, and hope, by thoroughly excavating the several mounds, to bring to light specimens which will doubtless help to prove the age in which they were constructed, and the similarity which existed between the manner and customs of the race who formed them, and the constructors of those placed in like positions on the shores of Denmark and Northern Europe.

J. M. JONES,

President of the Institute of Natural Sciences.

ABSTRACT OF THE FIFTH REPORT OF DR. KELLER

ON

LACUSTRIAN SETTLEMENTS.

FROM THE BULLETIN OF THE NATIONAL INSTITUTE OF GENEVA.

In January, 1854, certain works, undertaken on the shores of the Lake of Zurich, at Obermeilen, brought to view, with the mud and ooze from the bottom of the water, an assemblage of ancient remains, together with piles. Dr. Keller, president of the Archæological Society of Zurich, published in the spring of 1854 a first report respecting this discovery. It was a brief but lucid description, accompanied with numerous figures, and the conclusion was even then arrived at that there had existed in ancient times, at the point in question, habitations built upon pile-work. Discoveries of the same kind were rapidly multiplied in Switzerland, few savants possessing, in an equal degree with Dr. Keller, the art of guiding and encouraging others in the labors of research. His correspondence forms a connected course of instruction, strikingly recommended by the unaffected liberality which pervades it, and which naturally evokes a reciprocal spirit of frank communication in regard to all new facts and observations. To this concurrence of efforts, directed to different points, which, taken separately, would have been of little avail, we owe the rapid development of Swiss archæology; and it is this also which has enabled Dr. Keller to publish a second report on lacustrian habitations in 1858, a third in 1860, a fourth in 1861, and now the fifth, with which we are at this moment occupied. These several reports are all distinguished by an affluence of well-ascertained facts, and of accurate figures, as well as by the absence of those idle discussions and fantastic reflections which are still but too rife in matters of archæology. Nor is it a circumstance unworthy of notice that even our neighbors of Italy and Germany have contributed to swell this fifth report by valuable communications presented under their own names; for Dr. Keller is of that class of savants who conscientiously render to each whatever is his due, and willingly withdraw themselves from notice in order to give greater prominence to the merits of another.

Unfortunately Dr. Keller only publishes in German, whence his reports, though now and then containing an article written in French, such as the excellent paper of M. L. Rochat on the lacustrian habitations of the neighborhood of Yverdon, are too little known in certain countries. There should be a French publication recapitulating the labors of the savant of Zurich, but a natural repugnance is felt to undertaking such a work while progress and discovery are still in full career. We shall, therefore, confine ourselves to a simple review of the fifth report, which is before us.

This report commences with a notice of ten pages on the *Terramara de l'Emilia*, by M. P. Strobe, professor of natural history in the University of Parma, and M. L. Pigorini, a young archæologist of the city of that name. The German translation is from the pen of M. Strobel, who speaks and writes German perfectly well. Three plates, comprising eighty-nine figures, accom-

pany this notice, which resembles those given by Dr. Keller in its avoidance of all useless phraseology.

In the duchy of Parma there occur, in the level tracts bordering upon rivers, deposits of a peculiar nature, which have been for some time employed, under the name of terramara, in the culture of lands. They are accumulations of a marshy nature, interspersed with beds of river ooze, of charcoal and cinders, through the whole of which are thickly strewn the crushed bones of animals, pieces of wood, fragments of pottery, and divers objects in bone, in stone, and in bronze. It is apparent that man once inhabited these places, liable as they were to occasional submersion. At one point there was found, in good preservation, a floor built upon piles, which had been planted in a marshy soil beneath shallow water, which, by the accumulation of solid material, had since become dry land.

The bronze articles occuring in the terramara are hatchets, reaping-hooks, lance-heads, poniard-blades, hair-pins, a small bronze comb, chisels, and awls, the whole being of the kind met with in Switzerland and the north, and regarded as characteristic of the age of bronze. The pottery is coarse, composed of clay mingled with sand, rudely shaped by hand, without the use of the wheel, as is still practiced in villages of the Appenine in preparing utensils intended to resist the action of fire. The vases present a peculiarity, not as yet elsewhere observed, in being often furnished with small handles, drawn out into variously shaped horns and knobs, and sometimes ornamented with stripes. Spindle whirls, plain or striped, are of frequent occurrence. Among the objects of bone may be mentioned two combs, embellished with carvings in the manner of the bronze age, and among those of wood the remnant of a wicker basket. The remains of animal bones have been carefully studied by Professor Strobel, who, after having compared them with those of the lacustrian settlements of Switzerland, described by Professor Rutimeyer, of Bâle, has had the satisfaction of seeing even the most questionable of his decisions confirmed by the last-named savant. The species thus far recognized by M. Strobel are: remains of the bear, the wild boar, the roe-buck, and the stag; and, of domestic animals, the dog, the horse, the ox, the hog, the goat, and the sheep, all of them races occurring in the lakes of Switzerland. To this list should be added some remains of birds, and, among others, of the domestic fowl, with those of terrestrial and fluviatile mollusks, still found alive in the country. The vegetable kingdom has contributed various kinds of wood, wheat, *(triticum turgidum,)* beans, hazel-nuts, pears, apples, service-berries, acorns, and the capsules which enclose the seeds of flax. It would appear from the collective circumstances that the terramara represents what may be called the kitchen-middens *(kjækken-mædding)* of the age of bronze, formed in co-operation with the alluvium of rivers.

Lacustrian settlement at Peschiera, on Lake Garda, in Italy.—M. de Silber, Austrian officer of engineers at Verona, reports that, in dredging at the entrance of the port of Peschiera, remains of pile-work were found, entirely buried in the mud at the bottom of the water, while the mud itself contained numerous objects in bronze, of which Dr. Keller gives three plates of figures. These consist of poniard-blades, hair-pins of various shapes, hooks, or small fish spears, a knife, and some small remnants of clothing, all bearing much resemblance to those taken from the lakes of Switzerland. Among these objects from Peschiera are some of copper, which leads Dr. Keller to dissent from the generally received idea that the age of bronze, properly so called, had its origin in Asia, since Europe would then have had no age of copper, forming the necessary stage between the age of stone and that of bronze. Dr. Keller presents, in support of his opinion, a plate comprising the figures of twenty-eight objects of red copper, chiefly hatchets and coins, found in Hungary and Transylvania, and he adduces the testimony of a friend of his, who resided long in Hungary, and

who affirms that these objects of copper are frequent in the countries of the lower Danube.

Lacustrian settlements of the Untersee, that is, of the portion of the Lake of Constance to the east of the city of Constance.—For several years an extensive pile-work of the age of stone, situated near the village of Wangen, at one league and a half from Stein, had been used, with a view to the trade in antiquities, by one Lœhle, under the direction of Dr. Keller, who has spoken of this locality in previous reports. Recently M. K. Dehoff, employed in the customs of the grand duchy of Baden, has explored the whole Baden part of the Untersee, and his account, occupying nine pages, is given with the skill of a master, and the precision of a mathematician. Many of the observations already made at Wangen are here reproduced, but several interesting results of a general nature flow from them. In the first place, there is the absence, in all this region, of pile-works belonging to the age of bronze, all those explored up to this time having furnished, besides pottery, bone, buck-horn, &c., only stone, without any trace of metal, which does not import, however, that none will ever be found. Another curious remark is, that silex of foreign production occurs, unshaped and in abundance, at certain localities, denoting a place of fabrication, while elsewhere it is wholly wanting, as if the division of labor had existed, not only among individuals of the same settlement, but among the lacustrian villages, to some of which the preparation of instruments of silex, for the common supply, had been specially assigned. It is also a striking circumstance that in these settlements without metals are not unfrequently found hatchets of serpentine of excellent form, so ingeniously and even ornamentally wrought that we might be inclined to refer them to a later age, characterized by greater advances in art, and by the employment of bronze. On the other hand, such handles of buck-horn for the stone wedge as are found at Meilen, at Moossedorf, and elsewhere, are almost entirely wanting in the Untersee. Here the usual form of handle for the stone wedge was the branch, bent and notched with a ligature to retain the wedge in the notch. Two plates, with twenty-seven figures, accompany the memoir of M. Dehoff, comprising, among others, the plan, with sections, of the pile-work near Allensbach, the place of each pile being indicated, which gives, for the first time, a complete and correct idea of the subject. In concluding, M. Dehoff furnishes also some information respecting the prolongation, towards the northwest, of the Lake of Constance, called Ueberlingersee, which presents, in respect to lacustrian settlements, the same features with the Untersee.

The fascine-work of Nieder-Wyl, near Frauenfeld, canton of Thurgau.—Dr. Keller, while he gives the French term *fascinage*, calls it in German *packwerkbau*, corresponding somewhat to that which is known in Ireland under the name of *crannoge*. A small lake, or, more properly, a natural pond, filled with peat, was subjected to exploration. At one point the workmen reached, at a depth of from two to three feet, under the surface of the peat-moss, a collection of wood and solid matter, forming a sort of isle of about 20,000 square feet, around which there was a depth of eight or ten feet of the peat before attaining the ancient bed of the lake. This isle was ascertained to be an artificial construction, which had served as a foundation for habitations. To the selected point in the lake it seems that logs and boughs were brought, bound together in rafts, and loaded with sand to make them sink, piles being driven around to mark the limits of the construction, and the operation repeated till it rose above the surface of the water. A floor of logs, in close juxtaposition, was then laid upon sills regularly arranged, and this floor was covered with a layer of compacted clay, upon which the dwellings were erected. These dwellings were rectangular, being, on an average, twenty feet long and twelve wide. The walls, parts of which were still in place, were formed of logs split into rough boards, confined between stakes or posts planted vertically at suitable inter-

vals. In the corner of one of these dwellings there was found a hearth formed of unwrought flag-stones, still covered with coals and cinders. The floors, having sometimes sunk, at one point or other, to the extent of several inches, even a foot or more, the level had been restored by filling up the cavity. It would seem, in some instances, that the entire floor had sunk beneath the level of the water, and new ones been constructed above, since the remains of articles of domestic use or production occur between the two courses. The dwellings, which seem to have been covered with thatch, were distant from one another only two or three feet, and it is in these interstitial spaces, where the floors were more or less interrupted, that the remains of human industry have been chiefly discovered. This settlement bears no marks of having been destroyed by fire; it appears to have been voluntarily abandoned. At all events, its remains are the most complete and best preserved which have been yet discovered in Switzerland.

The constructions discovered by Colonel Suter, of Zofinguen, in the peat-moss of Wauwyl, much resemble those just described, only at Wauwyl they are more primitive and less skilfully combined, although those of Niederwyl belong to the age of stone, as well as those of Wauwyl. The researches at Niederwyl have disclosed hatchets of stone, wheat and tissues of flax, both charred, fragments of pottery, and bones of animals, which had served for food. We owe this interesting discovery to the zeal of M. Pupikofer, who has superintended the excavations made by M. Messikommer.

Lacustrian settlement near Zug, described by Professor Mühlberg, of Zug.—In the suburbs of Zug, on the road leading to Cham, workmen were digging the foundations of a house, when, at a depth of five feet, a dark-colored bed of decomposed organic matter was encountered, in which were found hatchets of stone, fragments of silex, hulls of hazel and beach nuts, apple-seeds and animal bones, together with the tops of stakes planted vertically, on somé of which still rested cross-pieces of wood. Here, there were evidently the remains of a lacustrian settlement of the age of stone, embosomed in the solid earth which had gradually encroached upon the lake. The bones have been examined by Professor Rutimeyer, of Bâle, and he has distinguished the cow, of that race which he names after the peat, the peat hog, the peat dog, the roe and the deer.

Settlement of Ebersberg, canton of Zurich.—In a sequestered spot, at the back of a hill called the Ebersberg, near the Rhine, ancient remains have been found, which M. Escher de Berg has described in vol. vii, 4th part, of the Memoirs of the Archæological Society of Zurich. M. Escher resumed his researches in 1862, and has drawn up an account of his explorations, which were continued for 64 days. This site has a peculiar interest, for it presents the remains of a settlement on terra firma, and an assemblage of objects entirely corresponding with those which characterize the lacustrian habitations of the age of bronze, for instance, in the lake of Bienne. Under 5 or 6 feet of detritus, an ancient surface of well-rammed clay was brought to light, and on this surface were discovered near one another the remains of two rectangular ovens, 5 to 6 feet long by 3 broad, formed of siliceous pebbles and clay mixed with much sand. Beyond these there was a pavement of pebble stones, and it was on these substructions that the bed containing antique articles immediately rested, while the thick mass of superincumbent humus was entirely destitute of them. In the bed spoken of, the very first excavations had yielded a crescent of stone skilfully cut. In the recent excavations a second crescent has been disclosed, but composed of baked clay, precisely like those taken by Colonel Schwab from the lake of Bienne, and which were probably used in the religious rites of the time. These later researches have also yielded: fragments of flint, wedges or hatchets of serpentine, stones for crushing grain; and, of bronze, two knives, some dozens of hair-pins like those of the lakes, several small chisels, an arrow-point, a number of rings and of plates of metal orna-

mented with lines. Other objects obtained are: a bead of glass or of blue and white enamel, such as we now have from the lakes of Bienne and Neufchatel; buck-horns carved; fossil teeth of the shark taken from the molasse of the country; spindle-whirls of baked clay; pottery, like that of the bronze-sites in the lakes; cones of baked clay, with a hole at top, designed doubtless as weights to stretch the threads in the process of weaving; and pieces of the clay facings of the walls of wicker-work, bearing the impression of the branches or osiers destroyed by fire. Bones of animals were by no means wanting, and they have been ascertained by Professor Rutimeyer to pertain to a cow of large species, to the hog, the goat, the deer and the roe-buck.

Lacustrian settlement of Robenhausen, at Lake Pfäffikon, canton of Zurich.— Of this notice has been taken in previous publications of Dr. Keller. M. Messikommer continues to make explorations, leading to interesting observations and to the discovery of objects, often of great curiosity, which, after having submitted them to the inspection of Dr. Keller, he offers for sale. This locality is situated in a moss, at the east end of the lake, which there had but little depth, and where the growth of the peat has by degrees advanced the limits of the dry land. To arrive at the bed containing piles and antique objects, it is necessary to remove some six feet of peat; this requires long continued exhaustion, but the objects are in a remarkable state of preservation. The report on recent researches is drawn up by M. Messikommer, who even indites some pleasing verses on the occasion. He has remarked that the objects are found more or less grouped, according to their nature. Thus, at certain points, charred cereals occur in abundance; elsewhere flax prepared for spinning; further on there may be flax woven or platted, and at still another place numbers of those perforated cones of baked clay which pertained to textorial operations. At one point M. Messikommer discovered that under the floor of the ancient dwelling there was a formation of peat from 2 to 2½ feet deep, beneath which was found another floor, still more ancient. We must infer that the place was long inhabited and during the age of stone, for not the least trace of metal has been met with.

The new acquisitions at Robenhausen, to which Dr. Keller has appropriated two plates, are: a canoe formed of the hollowed trunk of a tree, 12 feet long by 1½ wide, with a depth of 5 inches (the Swiss foot has 10 inches and is equivalent to 0.3 of a meter;) some well fashioned bows of yew wood; an arrow point of silex, still attached to its wooden staff by means of flax thread and mineral bitumen; a hatchet or wedge of stone fixed transversely in a wooden handle, somewhat club-shaped; another hatchet of stone fixed in a piece of buck's horn, which again was fastened transversely to the handle of wood. This last arrangement was also met with at Concise, but the stupendous impostures practiced at that locality throw suspicion on whatever comes from it, especially when it is known that the counterfeiters went so far as to cast their own fabrications into the lake, that they might be afterwards drawn up by the dredge before the eyes of the amateurs. At Robenhausen, divers articles of wood also have been collected, such as knives, basins, implements which served perhaps for beating butter, and large spoons like those for skimming milk. Among articles of flax, recently obtained, may be mentioned a portion of a girdle or ribbon quite skilfully woven, so as to present a small figure in squares of very neat appearance; also remnants of fishing-nets, with meshes measuring 0.05 of a meter on the side; and, lastly, a bit of cloth to which a pocket is attached by sewing.

Settlement in the lake of Bourget, in Savoy.—Baron Despine having drawn attention to a pile-work in the lake of Bourget, the Savoyard Society of history and archæology caused reseaches to be made, under the direction of MM. Despine and Delaborde. M. Rabut Laurent has given an account of them, in the Bulletin of the above Society, from 1861 to 1862, second number, p. 44, and

this report is here republished by Dr. Keller. At the point in question there have been found articles of pottery, calcined bones, a stone hammer, a small bronze ring, ears of wheat, acorns, hazelnuts, cherry-stones, grains of millet, and, what has not been yet met with in Switzerland, husks of chestnuts. Professor Desor also has made explorations in the lake of Bourget; and M. Louis Revon, the zealous and able director of the museum of Annecy, has commenced them in the pile-works of the lake of Annecy.

Lake of Neuchâtel, new discoveries of Colonel Schwab, 4 plates, comprising 71 figures.—The indefatigable colonel has caused dredgings to be executed at several points and has considerably enriched his admirable collection at Bienne. Certain objects reappear in indefinite numbers, such as hair-pins of bronze, but from time to time new and curious articles repay the zeal of the antiquary. We may distinguish of this class, a wheel of cast bronze, 0.49 of a meter in diameter; it has four radii, which, equally with the perimeter, are hollow. The nave, also hollow, is prolonged on both sides, making its entire length 0 50 of a meter. Near this wheel, thirteen small objects of the crescent-moon shape were found, each with a handle perforated at the end, as if to suspend the object, which is of bronze cast in a single piece. These, as well as the wheel, were perhaps employed in some religious ceremony. Similar small crescents appear also in the exquisite collection of Madame Febvre, of Chiseul, at Mâcon, a French lady, whose 83 years place in stronger relief the artistic discrimination, as well as the rare and high-bred courtesy of the venerable owner. Among the new acquisitions of Colonel Schwab we should further specify a sling of platted flax, exactly like one brought from the Sandwich Islands, and to be seen in the museum of Berne; also several beads of amber, and others, oblong in form, of blue glass or enamel, around which is encrusted a spiral of white enamel. These glass beads have been met with at four stations, whose characteristics clearly assign them to the age of bronze. In Mecklenberg, also, beads of blue glass, but of simple formation, have been twice found in tombs of that age. It is to the age of bronze, then, that we must refer the appearance of glass, but only in the shape of such beads; and even these are extremely rare at that epoch, at least in countries north of the Alps.

Of all Colonel Schwab's discoveries, the most curious is the product of a station of the age of bronze near Coataillod, being a dish in terra cotta, fashioned by the unassisted hand, having a diameter of 0.39 of a meter and a height of 0.4 of a meter, and inlaid on the inner surface with small plates of tin. These plates, which are themselves embellished with carved lines, are so arranged as to form a geometrical design surprisingly rich and ingenious, comprising among others a circuit of figures, which recall those, in the Greek manner, seen on Etruscan vases. The surface of the vessel had been blackened and rendered lustrous by being rubbed with graphite. It has not been ascertained by what means the tin was made to adhere to the surface of the material.

The above notices are followed by some account of the stations of pile-work in the lakes of Sempach, Baldegg and Mauen, and by a brief memoir of professor Deicke on the researches made by M. Ullersberger of Uberlinger in the lake of Constance. The publication of Dr. Keller concludes with 7 pages of remarks on the book entitled *Lacustrian Habitations of Ancient and Modern Times, by F. Troyon.* After having long kept silence, Dr. Keller at length raises his voice to rectify the errors and refute the absurdities of the book in question; a book which tends to induce obscurity, where it would be so desirable to proceed by sober investigations, set forth in simple and precise terms. Dr. Keller incidentally notices that his own reports have been entirely absorbed in the work of M. Troyon. The distinguished savant of Zurich could say no more, for he is not one to complain of having been unfairly laid under contribution. In the remarks spoken of, he shows that the collective phenomena of lacustrian settlements seem to evince a gradual and peaceful development of

civilization in Switzerland, from the age of stone to the Roman epoch, without an indication of violent social convulsions or industrial revolutions, suddenly superinduced by external and intrusive influences. It is doubtless picturesque to burn periodically, as M. Troyon does, all the lacustrian cities and to massacre their population. But it is more rational to recognize, as Laplace did at the close of his long and brilliant career, that what we know is little, while what we do not know is immense!

A. MORLOT.

P. S.—The seventeenth plate of Dr. Keller is not alluded to in the text of the report; it contains plans of the lakes of Neuchatel, Bienne, Morat and Sempach, with an indication, according to the researches of Colonel Schwab, of all the lacustrian stations discovered, distinguishing them as respectively dating from the age of stone, of bronze, of iron, or finally from the Roman epoch, for there are a few where Roman objects have been found. Professor Vogt has published a work on man, *Vorlesungun neber den Menschen*, in which he severly criticises certain parts of M. Troyon's "Lacustrian Habitations," upon which Dr. Keller had not animadverted; and the central organ of German archæology, published at Nurnberg, *Anzeiger für kunde der deutschen vorzeit*, equally takes ground against M. Troyon's book. (See Beilage, No. 10, October, 1863, page 373.)

AGRICULTURAL IMPLEMENTS

OF THE

NORTH AMERICAN STONE PERIOD.

BY CHAS. RAU, OF NEW YORK.

My collection of Indian stone implements contains a number of specimens remarkable alike for large size and superior workmanship, which, to all appearance, have been used for agricultural purposes by the aborigines of this country; and, as no description of similar relics has appeared as yet in any modern work on North American ethnology or antiquities, a notice thereof might be acceptable to all who take an interest in the former condition of the aboriginal inhabitants of North America.

The implements in question are of two distinct forms, represented in the woodcuts, figures 1 and 2, and may be classified, from their shape and probable application, as *shovels* and *hoes*. The material from which they are chipped, and which I never succeeded in discovering *in situ*, is invariably a very hard flint of a bluish, gray, or brownish color, and a slightly conchoidal fracture, and quite unlike that variety of flint of which the arrow and spear heads occurring in the west are usually made.

Fig. 1.

Fig. 2.

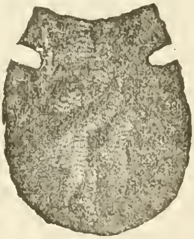

Fig. 1 represents one of the shovels in my possession. Like all other specimens of this kind, it is an oval plate, flat on one side and slightly convex on the other, the outline forming a sharp edge. It measures above a foot in length, a little more than five inches in its greatest breadth, and is about three-quarters of an inch thick along the longitudinal diameter. The workmanship exhibits an admirable degree of skill. Besides the specimen just described, which was discovered in a field near Belleville, St. Clair county, Illinois, I possess two others of similar shape and workmanship. The one of these last named I found myself within sight of the celebrated Cahokia temple-mound in Illinois, in the construction of which it may have assisted centuries ago; the other was dug up in 1861 in St. Louis, while earthworks were built by order of General Frémont for the protection of the city against an apprehended attack of the southern secessionists. When attached to solid handles, these stone plates certainly constituted very efficient digging implements.

Fig. 2 illustrates the shape of a hoe. This specimen, which was obtained from a burial-mound near Illinoistown, opposite St. Louis, is seven and a half inches long, nearly six inches wide, and about half an inch thick in the middle; the round part is worked into a sharp edge. Another specimen of my collec-

tion, of equal workmanship but inferior in size, was found, after a heavy rain, in a garden in the city of Belleville. The fastening to a handle was facilitated by the two notches in the upper part, and, in order to constitute a hoe, the handle was doubtless attached in such a manner as to form a right or even an acute angle with the stone plate.

If the shape of the described implements did not indicate their original use, the peculiar traces of wear which they exhibit would furnish almost conclusive evidence of the manner in which they have been employed; for that part with which the digging was done, appears, notwithstanding the hardness of the material, perfectly smooth, as if glazed, and slightly striated in the direction in which the implement penetrated the ground. This peculiar feature is common to all specimens of my collection, and also to the few which I have seen in the possession of others. They seem to be rather scarce, and merely confined to the States bordering on the Mississippi river. Dr. E. H. Davis, of New York, has none of them in his excellent and comprehensive collection of Indian relics, and, consequently, does not describe or represent them in his work on the "Ancient Monuments of the Mississippi Valley," forming the first volume of the Smithsonian publications; nor am I aware that Mr. Schoolcraft has mentioned them in his large work on the North American races.

A passage in the "History of Louisiana," by Du Pratz, refers, doubtless, to the implements described by me as hoes. In speaking of the agricultural pursuits of the Indians of Louisiana, that author observes, they had invented a hoe, (pioche,) with the aid of which they prepared the soil for the culture of maize. "*These hoes*," he says, "*are shaped like a capital L; they cut with the edge of the lower part, which is entirely flat.*"* It is true, he does not mention of what material this "lower part" consisted, but we may safely infer that it was stone, the substance from which the aborigines of North America manufactured nearly all their implements of peace and war. They had no iron, and the scanty supplies of native copper, derived from the region of Lake Superior, were almost exclusively used for ornamental purposes.

The fact itself that simple agricultural utensils of Indian origin are occasionally met with is by no means surprising, for we know from the accounts of the early writers that many of the North American tribes raised maize and a few other nutritious plants before the arrival of the Europeans on this continent. Maize was, however, their principal produce, and that on which they mainly depended. In describing the ill-fated Mississippi expedition of De Soto, Garcilaso de la Vega speaks repeatedly of the extensive maize fields of those Indian tribes through whose territories that band of hardy adventurers passed. During an invasion of the country of the Senecas, made as early as 1687 under the Marquis de Nonville, all their Indian corn was burned or otherwise spoiled, and the quantity thus destroyed is said to have amounted to 400,000 minots, or 1,200,000 bushels.† It is even asserted by Adair, that the colonists obtained from the Indians "different sorts of beans and peas with which they were before entirely unacquainted."‡

From these and other facts, which need not be cited in this place, we learn that the North American Indians generally, though warriors by disposition and hunters by necessity, had, nevertheless, already made some steps towards an agricultural state. But the events that happened after the arrival of the whites, instead of adding to their improvement, served only to lower their condition, and reduced them, finally, to the position of strangers in their own land.

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* Ces pioches sont faites comme une L capitale; elles tranchent par les côtés du bout bas qui est tout plat.—*Histoire de la Louisiane*, par M. Le Page du Pratz, (Paris, 1758,) vol. ii, p. 176.

† Documentary History of New York, vol. i, p. 238. This estimate may be somewhat exaggerated.

‡ The History of the American Indians, by James Adair, (London, 1775,) p. 408.

ANCIENT FORT AND BURIAL-GROUND.

PERRY CITY, NEW YORK, *January* 25, 1864.

The remains of an ancient fort and burial-ground exist about one-half mile northwest of Waterburg, a small village in the town of Ulysses, Tompkins county, New York. When the country about here was unsettled, some sixty years ago, the remains of this monument of a former period was plainly to be seen. The fort (by which name it is called and generally known about here) was situated on a rise of ground some twenty-five or thirty feet above the level of a stream of water—Tanghanic creek—large enough, when the country was new, to run a saw-mill four or five months of the year, the creek forming the southeastern boundary of the lot.

The "fort lot" contained eight or ten acres, perhaps, and around its eastern and northern sides an embankment was thrown up several feet in height. At this time it is not more than one foot, or near that; but, before it was ploughed, it was considerably higher than at present. At the northwest extremity of this embankment a ditch was dug at right angles to it. Around the outside of the embankment posts were set, which, perhaps, served the same or a similar purpose to that which our fence-posts do now. These posts were set into the ground to a depth of three feet, and judging from this we should be led to conclude that they extended above ground eight or ten feet. On the west side there were three rows of posts, but no embankment that could be discovered. But it is very probable that the ditch, of which I have before spoken, extends the whole length of the west side, though it can now be traced but a little way. At the northeast and southeast corners there were gate-posts set, where the gates were situated, which afforded egress and ingress to the camp. The southeast gate was calculated to afford a direct passage to the stream of water before mentioned, while the other one led directly to a burial-ground. On the southern and southeastern sides there is a bank fifteen or twenty feet in height, and pretty steep. Posts were here set part way down the bank so that a bridge might be formed over the bank for some purpose besides preventing any one from entering from that side. Mr. Jonathan Owen, (an aged farmer who resides near the fort,) from whom I have most of my information, thinks that the inhabitants of the enclosure had access to the creek by an underground passage. Let this be as it may, it is very evident, from the appearances around, that they guarded against enemies on all sides, thus showing that some other party or nation was hostile to them.

About sixty years ago everything that I have described was distinctly visible. Parched Indian corn was seen in considerable quantities in various places. The corn, in fact, was burnt black, and everything else showed that the whole structure had been destroyed by fire. If it had rotted down or decomposed in the ordinary way, it is not probable that the wooden part of the fabric would have remained many years. The part of the posts that entered the ground had been burnt to charcoal. It is probable that large quantities of Indian corn which were put up for future use were destroyed by fire. Mr. Owen stated that, "after digging through about two inches of loose dirt," he came to a bed of about the same thickness of bones, oyster, and clam shells, and a considerable quantity of earthenware. The bones were principally deer's bones. Below this was a bed of ashes of nearly the same thickness. The remains of their earthenware showed that they had made some progress in the arts.

When the embankment around the northern and eastern sides was ploughed it was found to be composed of a loose mucky earth, very much resembling earth formed mostly from rotten wood. This led Mr. Owen to the conclusion that the embankment was formed of logs covered with earth. Its being covered with earth to some depth would prevent the logs from taking fire when the structure was destroyed in that way.

A part of the embankment extends into the woods on the north side, and on it are growing several trees, one of them a pine tree $3\frac{1}{2}$ feet in diameter. This tree has undoubtedly grown where it is since the embankment was made. The tree must be several centuries old. This, and in fact everything around it, testifies to the comparatively great antiquity of the fort.

A few rods to the west of the enclosure, on a knoll, there were two burial-grounds, where the dead bodies of the inhabitants were deposited. Sixty years ago, according to my father, "a hundred graves could be counted in a row." These burial-grounds were quite extensive, embracing not less than two or three acres. In a northeast direction, about fifteen rods from the fort, was another burial-ground. The northeast gate, as before mentioned, led directly to this one. This burial-ground contained at least half an acre. In all of them tho bodies were as thickly deposited as they conveniently could be. The last burial-ground mentioned is still visible, it being in the woods; but the other two have been ploughed, so that they cannot be distinguished at present. In the one that is now distinguishable, I have assisted in digging out several graves. In some, bones were found; while in others, nothing of the kind were seen. Wherever there is a grave the earth is sunk a little. In the first one that was opened we found the thigh-bones, hip-bones, arm-bones, and various other smaller ones. A jaw-bone and several teeth were found, but no hair. We used nothing but our hands to throw out the earth with; otherwise, it is probable, we should have found more things. The earth was very loose, and it was, consequently, easily thrown out. The depth of the grave was about $3\frac{1}{2}$ feet. One grave, in which several bones were found, was under a root of the stump of a large pine tree. This tree was, perhaps, from three to six hundred years old, and it is probable that it has grown there since the grave was made.

All things indicate that these people were buried in a sitting posture. The graves are very short, not being more than four feet in length. Also the jaw, hip, and thigh bones were all found together, just as they naturally would be if the body was buried in a sitting posture.

Various little trinkets have been found on the "fort-lot" at different times. A great many arrow-points have been found there, made of the hardest flint stone. Stone hatchets, or axes, have also been found. Several years since, a neighbor, Mr. David Farrington, found a pipe there, probably used for smoking tobacco; the stem was not very long, but of a sufficient size to admit a wooden stem of any length; the pipe-bowl had the face of a frog formed on it.

Within three miles of here there are three other similar forts to the one which we have here described.

DAVID TROWBRIDGE.

REMAINS OF AN ANCIENT TOWN IN MINNESOTA.

Itasca, Anoka County, Minnesota,
November 25, 1863.

Dear Sir: Presuming that your Institution is the proper one with which to file a report of new discoveries, I take the privilege and pleasure to inform you that indications are favorable to encourage the belief, that upwards of one hundred years ago there existed at the mouth of Crow river, where it empties into the Mississippi, 24 miles above the Falls of St. Anthony, a town comprising at least seven hundred inhabitants. I have commenced collecting the articles that have been found, with the intention of forwarding the same to you if you desire me to do so.

We presume that the village was destroyed by fire of an enemy, for these reasons: we find the outlines of the buildings forming ridges of earth, under which are ashes, indicating fire; we also find human bones near the surface, which leads to the belief that they were not buried.

As to proof of the age of these ruins, trees have been cut down having one hundred rings, which were growing inside of the piles of ashes.

That they were at least civilized, is shown by our finding the locality of a blacksmith's forge, where the cinders, bits of iron, &c., were plenty. Each house was furnished with a fireplace of stone, the foundations of which are easily found.

Among the articles I now have, though somewhat decayed, are knives, forks, a fish-hook, piece of china bowl, piece of looking-glass, very long and well made wrought nails, part of an iron hinge, part of a clay pipe, strips of copper, one knife of extra fine quality of steel—has the name of "Pelon" on the blade. If among your antiquities you have any cutlery bearing the same mark, it may perhaps assist us to ascertain the direction these ancient settlers came from.

If you deem this information of any value, and will give me any directions regarding further explorations and the manner in which you wish the articles sent you, I will, as soon as the frost in spring will permit, turn over the ashes in several more places, in hopes to find some record to add interest to the discovery.

Can you gather any information by examining a jaw-bone of a human skeleton? I have one I can send, found near the forge.

I remain yours, most respectfully, in haste,

O. H. KELLEY.

P. S.—I am the oldest white settler, with one exception, in this neighborhood, having been here since January 1850, and have seen the forest cleared from the ground where these ruins are found, and the present little town of Dayton built up. Senator Ramsey will vouch for my being an old settler here.

ANCIENT RELICS IN MISSOURI.

WASHINGTON, *March* 14, 1864.

MY DEAR SIR: I promised you some time ago a description of some ancient relics of pottery from the mounds of Missouri, but that promise has remained unfulfilled up to this time.

Accompanying this note are photographs of three vessels:

1. Front view } of probably a priest, or some official personage, if we re-
2. Profile view } gard the head-dress as a badge of office.
3. Back view } of a captive, bound, perhaps, for immolation.
4. Profile view }
5. A plain vessel without any ornamentation.

These vessels are about twelve inches in height and are composed of clay slightly burnt, and are without any glazing. The interior is hollow, and the orifice in two instances is at the side, and in the other at the top. The thickness of the crust is about one-fourth of an inch. I regard them as water-coolers; the texture being such as to retain water for a considerable time, and also to allow evaporation from the exterior surface.

I think you will agree with me that the ancient sculptor exhibited considerable skill in moulding. The proportions of the features are not very grossly exaggerated, and he possessed sufficient skill to delineate the traits characteristic of his race. Those traits belong not to the North American Indians, but, I think, to the Peruvians. The fillet on the head I am disposed to think was made of cloth. I hand you specimens of ancient weaving, which I have heretofore described. (*Vide* Trans. Am. Asso., Albany meeting,) [1855?]

These specimens were taken from mounds in Mississippi county, Missouri, by the late Sylvester Sexton, of Chicago, and are now in the possession of his

widow. From Mr. Stevens, who assisted in the exploration, I drew the following particulars:

There are several mounds scattered throughout this country, but the largest group, and that from which these relics were taken, is on section 6, township 24, range 17, extending over about ten acres of ground. The mounds vary from 10 to 30 feet in height, and many of them, on exploration, have yielded relics.

The most convenient point of approach is from Columbus, Kentucky, being about eight miles distant, and about seven miles from the battle-ground of Belmont.

Yours, very truly,

J. W. FOSTER.

MOUND IN TENNESSEE.

SALEM, MARION COUNTY, OREGON,
December 12, 1863.

SIR: We write on a subject of some though not of great moment. That subject is this: On a mound, in East Tennessee, on Lick creek, near its junction with the Nolechuckey, in Greene county, six miles north of Warrensburg. It is some twenty-five or more feet high, covers an area of half an acre or more, is cone-like or round, is quite steep, and flat on its apex. It is a made mound, is of loam, and in the bottom next the creek. There is an excavation near, showing, evidently, that the earth removed is that of which is formed the mound. This mound is full, so far as examined, of human bones and carbonized wood. The bones lay irregularly, and seem to have been thrown in promiscuously. We think this a cemetery, or burial of slain in battle. The skeletons are larger than our race; are yellow and firm and strong when disinterred, but soon crumble on exposure. The apex is flat and sunken in the centre. Our informant, Mr. Isaac W. Bewley, brother to the martyr, Anthony Bewley, dug down some three feet, on top, and came to a burnt, smooth surface, under which, in sinking, he found large pieces of charcoal and considerable ashes. Mr. Bewley's father settled, or rather bought the place, some fifty years ago. How long it had been settled before we are not informed. The cause of our informant digging down on its top was from mere curiosity. This mound has no name that we know of. We have given you its location, hoping you may make known this, we think, important matter. The opening of this mound might lead to more than mere conjecture concerning a once enlightened race. The earth seems to have been *dug* and *elevated for a tomb*. This shows some *advance* in the race who did it. An excavation might *reveal* important facts. We think the mound should be examined. What has an examination of the lacustrian cities led to? To important results to the archæologist. And might not some good result from an exhumation of the *remains in this mound?* We think so, and therefore urge it.

We have written this for the cause of science—the light that may flow from an examination of this Lick creek mound, near the junction of Lick creek with the Nolechuckey.

And now another matter: We have here some relics of Indians, as stone mortars and pestles, arrow points, stone axes, stone scrapers, or knives, &c. Would these be of any use to the Smithsonian Institution? If so, please write me at your earliest opportunity.

There are mounds in this country, too, and if you desire it we will write you about them.

We are, &c.,

A. F. DANILSEN.

JOSEPH HENRY,
Secretary Smithsonian Institution, Washington city, D. C.

PURPLE DYEING, ANCIENT AND MODERN.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION FROM THE GERMAN PERIODICAL, "AUS DER NATUR," &c.

The idolatry of classical antiquity finds its chief antagonism in the natural sciences. It would be easy to show how many illusions, nestling in the heads of the admirers of the olden time, have been dispelled by modern chemistry alone; and, although our present purpose is to deal with two objects of subordinate importance, yet these also serve to show how very broad is the line of separation between our own times and the remote ages, to whose languages and ideas so much of the time and training of our youth are commonly devoted.

The colors of *azure* and *purple* were among the most highly priced as well as the most highly prized productions of antiquity. The former was sold for its weight in gold, and the latter was especially reserved for the noble and the powerful; its use was in some ages even forbidden to all beneath those of the highest rank on pain of death. Science and art have wrought here a striking change; being no longer limited to the direct gifts of nature, we are able, from the most apparently unpromising raw material, to furnish for the use of the whole community what could then be but scantily produced for the ruling few The contrast is certainly suggestive.

As early as three hundred and fifteen years before the Christian era, Theophrastus drew a distinction between natural and artificial azure, the latter of which, he tells us, was manufactured in Egypt. It seems most probable, however, that the terms natural and artificial indicate in this case only the greater or the less degree of care with which the color was prepared from the beautiful stone which we call *Lapis lazuli*, to which the ancients gave the name of sapphire. While in some cases the stone was merely reduced to a fine powder, in others, probably, the coloring matter was more carefully separated, as is done in our own day.

The Lapis lazuli, or sapphire, is found in the least accessible parts of Little Bucharest, Thibet, China, and Siberia, in layers or strata of granite or limestone. Of old, as at the present day, it was polished and wrought as a gem, and it is almost the only member of the large family of gems that has an intrinsic value. This distinction it owes to the fact that, in addition to its great beauty, it yields for the use of the painter one of his most beautiful colors, which, moreover, is unaffected by air or heat; that color is ultramarine.

As lately as the commencement of the present century, ultramarine, or azure blue, was not simply a fine powder of the gem, but the result of a long and troublesome process. The stone was first broken into small pieces, and even this first step in the process was no easy one, the stone being exceedingly hard. The pieces, of the size of a hazelnut, were cleaned by means of lukewarm water, then made red-hot, and afterwards slacked in a mixture of water and acetic acid. The cohesion of the particles is so great that this process must be repeated from six to ten times before the mineral can be transformed into a fine powder. It is afterwards rendered still finer by trituration with the muller stone of the painter, having been first mixed with water, honey, and dragon's blood, then treated with the ley of the ashes of the grapevine, and finally dried. The powder is next compounded into a mass with turpentine, rosin, wax, and linseed oil,

melted together, and kneaded under water. By this process the fine powder is washed out, and in time sinks as a sediment in the liquid. The mineral yields not more than one-fourth of its weight of coloring material.

Up to a very recent time Italy continued to be the chief, as it had been the original, manufactory of ultramarine, and thence the finest shades were derived. The tediousness, the difficulty, and, consequently, the costliness in both time and money of the old process of producing ultramarine from the Lapis lazuli, naturally excited great desire among scientific chemists to find some cheaper and readier artificial means of producing that color, doubly precious to the painter for its beauty and its permanency; but so invariable from different causes were the failures of all attempts in that direction that the solution of the problem was well nigh despaired of, when hope was as suddenly as accidentally revived. In 1818 it happened that in France a sandstone furnace for the melting of soda was taken down, and a beautiful colored substance, never seen there before, was discovered. It was remarked, that formerly the furnace for the melting of soda had always been constructed, not of sandstone, but of brick. The mass of matter thus discovered was examined by Vauquelin, who observed in its appearance and composition points of great resemblance with ultramarine; but still no clue offered itself to guide him through the perplexities of the investigation. Similar observations were made in other soda manufactories, as, for instance, by Hermann, in Schönbeck, who had thrown away above a hundred weight of the colored mass found in a similar furnace when the latter was pulled down; and by Kuhlmann, at Lille. We shall not venture to decide whether or not the "blue material," mentioned by Göethe in his "Italian Travels," (1781,) as being taken from limekilns in Sicily, and used for the adornment of altars and other objects, was homogeneous with this product of the soda furnace, and whether both were, in fact, an artificially and accidentally produced ultramarine.

The question still remained unanswered, how was this substance in the case of each furnace produced? In what did it originate? At length, in 1828, the solution of this important question was found and published by Professor C. Gmelin, of Tuebingen. During eighteen years he had been occupied with researches on the "Lapis lazuli" and its kindred minerals, the products of the volcanic eruptions of Vesuvius. Reflecting on the recent circumstance, he was led to believe that, notwithstanding there had been so many unsuccessful attempts, the production of an artificial ultramarine was not an impossibility. Further study of the natural coloring substance disclosed to him the sulphurous portion of the components, and holding that clue he at length succeeded in producing a most brilliant ultramarine.

At about the same time, another German chemist, the well-known "Doebereiner," had a glimpse of the true nature of the coloring principle of ultramarine. He was the first positively to assert that it was to be attributed to sulphur alone. He obtained, however, a mere glimpse of this beautiful discovery, other occupations preventing him from following it up. A very few more experiments, and he would have been completely in possession of it. Gmelin was scarcely more successful, though the absence of this additional jewel in his scientific crown was owing to a different cause. It is not in the nature of a true savant to place his talent at usury, or, in plainer terms, "to make money by it;" though now and then doubtless, in these days of extravagant projects, it is not impossible to find a savant at the head of some speculating manufactory, to the success of which his reputation gives a substantial guarantee. Men of science of this kind are certainly much sought after by industrial speculators, yet the exceptions do not greatly affect my assertion as to the general disinterestedness in this respect of the German savant.* He, for the most part, when, in the

* This characteristic is by no means confined to German savants, but is shared by most men of science in all countries.

course of his researches and experiments, a discovery has been made which may be rendered available for utilitarian purposes, forbears to make a secret of it, publishes it without reserve, and leaves the pecuniary harvest, large or small, to be reaped by others. Is this because the enthusiastic savant has so little worldly wisdom, or so exclusive a desire of reputation? Gmelin's own words may, perhaps, help the reader in forming a reply to this question. He says: "I have thus mentioned all the circumstances which must be kept in view in the manufacture of artificial ultramarine, and I have also added some hints for the use of those who may make it their object to manufacture this color on a larger scale. I have now only to desire that others in like manner may unreservedly publish their experience on the subject, so that the production of this article may, as early as possible, attain to the highest degree of perfection. We cannot, it is true, when an important technical discovery has been made, which promises large profits, fairly blame any one for keeping it a secret until he has achieved that great and justifiable aim of all mankind, security against want; but beyond this, no one has a right to maintain secrecy that he may secure gain. And it is very much to be regretted that by the withholding of so many discoveries (often buried with those who make and conceal them) science has been hindered in its progress, and an obstacle thrown in the way of the noblest object of man, that, namely, of increasing knowledge and diffusing civilization." Such, literally, was the practice of Gmelin. While at Paris, in 1827, and previous to the publication of his discovery, he unreservedly communicated his ideas on the artificial production of ultramarine to several chemists, especially to Gay Lussac. And, behold! on the 4th of February, 1828, Gay Lussac made a report to the French Academy that Guimet, at Toulouse, had succeeded in manufacturing ultramarine of all kinds. Did the discovery originate in the open and disinterested communication of Gmelin, or did it not? Who shall decide? Guimet, it is but just to say, warmly defended himself against such a suspicion; he affirms that he was prompted to his experiments by the examinations of *Lapis lazuli*, made by Desormes and Clement, and claims that he had produced artificial ultramarine before Gmelin's visit to Paris.

Whether the method of Guimet is essentially different from that of Gmelin cannot be determined, for, while the latter published his discoveries with every particular, Guimet, on the contrary, has kept his method a secret to the present day. In so far as profit is concerned, Guimet, it must be confessed, has maintained the advantage over Gmelin, and France over Germany; for Guimet forthwith made his discovery lucrative to himself and others. As early even as the same year, 1828, he had erected a manufactory at Paris for the production of artificial ultramarine, which he sold at two dollars and sixty-six and a half cents per pound, while the natural article was a little more than double that price. Guimet succeeded in having his product adopted for the painting of the beautiful ceilings of the museum of Charles X, and thenceforth his fortune was made. In 1834 the price had risen to from four to five and one-third dollars per pound, but in 1844 had again fallen, and ranged from two and one-sixth to two and one-third per pound, though the best quality for oil painting was still sold at six dollars and forty cents. The cheapness of the ordinary article enhanced the demand, and the product of Guimet's factory speedily rose from twenty thousand to one hundred and twenty thousand pounds, of which twenty thousand pounds were exported to foreign countries. Not only did Guimet amass immense wealth; he was the recipient also of many public honors. From the French "Society for the Encouragement of Industry" he received a premium of 5,000 francs, and medals from various French industrial exhibitions; and this as early as 1834, when the real importance of this eminent discovery could have been scarcely appreciated. In 1851, at the London exhibition, Guimet received the large gold medal.

In Germany the manufacture of ultramarine proceeded at a far slower and less profitable rate, though the directions published by Gmelin would have amply sufficed for manufacturing on a much larger scale. He already knew that the proportions of silicious earth, natron, and potter's clay might vary to a certain extent without at all affecting the result; and he had also found that the production of artificial ultramarine requires two distinct operations, viz:

1. The production of the so-called green ultramarine; and,
2. The transmutation of the green into the blue article by roasting the former, while allowing the access of air.

This latter necessity he was taught by the accidental bursting of a crucible. His observation of this accident enabled him to master the whole process, and conduct it to any desirable issue. To the manufacture on an extensive scale, however, the condition insisted upon by Gmelin of perfect or chemical purity in the silicious earth and the potter's clay employed, continued to present an embarrassing obstacle on account of the delay and difficulty in bringing the material to that state.

It is true that he had himself raised the question whether the production of the two expensive materials, both of them being components of potter's clay, might not be dispensed with, and he experimented upon various specimens of tolerably pure clay containing the maximum of 4½ per cent. of iron. But he considered the results of the experiments unsatisfactory, on account of the presence of even such a proportion of iron. From a porcelain clay containing very little iron he obtained, indeed, a very beautiful ultramarine, which he considered quite fit for oil painting, especially for landscapes. But even this product could by no means be compared to the natural and most beautiful kind. The artificial article always retained a scarcely perceptible tinge of green and gray; while the positive red, on which depends the peculiar brilliancy of the natural ultramarine, was wanting. This difference was especially noticeable when both pigments were rubbed in oil. The circumstance that Gmelin aspired to the highest excellence, and would not content himself with mere mediocrity, was an obstacle to the introduction of this article into German industry, and restricted its use when it was introduced. Still, the first German manufactory on the principle of Gmelin's process commenced working in 1834, under the management of Leverkus, of Wermelskirchen, and very soon occasioned a great change alike in the price and the popularity of the article.

In 1832, the celebrated French chemist, Dumas, in his "Manual of Chemistry," had expressed the opinion that chemical purity of materials might very well be dispensed with in the manufacture of artificial ultramarine, and that common clay might be used, provided it did not contain too much iron. Professor Engelhardt, of the Polytechnic School, Nuremberg, while translating the works of Dumas into German, was especially impressed by that statement, and was induced thereby to make new experiments, but his labors were terminated by death before he had obtained any positive and satisfactory results. His assistant and successor, Leykauf, continued the deceased professor's experiments, and was fortunate enough to succeed, where all previously had failed. By means of potter's clay, Glauber's salt, and coal, he manufactured the most beautiful ultramarine, in the renowned manufactuory of Ley Rauf, Heyne & Co., at Nuremberg; and in a very few years the firm counted its wealth by millions. Nowhere else has this branch of industry acquired such an extension; being conspicuous even among the diversified activities of Nuremberg, and justifying, therefore, a brief description in this article.

In the vicinity of the Nuremberg railroad depot, the attention of the observant traveller is pretty sure to be attracted by a stately and spacious mass of buildings of white and red sandstone. The long rows of structures, with their streets and yards, cover a space of some eighteen acres. Surrounded as the

whole is by a rampart, one might at first fancy himself to be looking upon a fortress. But the smoke from numerous tall chimneys would speedily correct this error and betray the abode of ingenious and successful industry. It is to be regretted that visitors are rigidly excluded from the interior of this industrial hive; a useless exclusion, as the manufacture of ultramarine can no longer by any possibility be considered a secret. The visit of the King of Bavaria, in 1855, to this equally interesting and important factory, so far lifted the veil that we possess something like a reliable description, instead of the strange surmises which were previously in circulation with respect to it. On a first glance at the exterior we perceive that the vast erection has been built piecemeal, additions having been made from time to time to meet the necessities of the increasing business. It required the long period of seventeen years to render the whole what it now is—a structure heterogeneous, indeed, in appearance, but really possessing the highest conceivable adaptation to the purposes for which it was designed.

Three rows of the buildings are devoted solely to the preparation of the raw material, the motive power consisting of two steam engines conjointly possessing a thirty-eight horse-power. So various and well contrived are the stampers, crushing and sifting machines, &c., which are set in motion by these various works, that a small amount only of human labor is required to furnish abundant raw material to employ elsewhere a vast number of hands.

Groups of buildings surrounding those just mentioned contain water-works, and consist of five divisions of vaulted galleries, supported by iron pillars. Near these are the drying stoves. Close by these three principal divisions are the buildings for storing, packing, and weighing, and the clerks' offices and repairing shops. Here is a scene of continual activity, the human labor being greatly aided by a high-pressure steam engine of twenty-horse power. The communication between these various and extensive buildings is facilitated by a railroad six thousand feet, or considerably above an English mile, in length, crossing from east to west, and from north to south, and similar tram roads of timber connect the buildings in the upper stories. The iron railroad leads to the depot of the public railroad; thus placing the factory in easy and speedy communication with the principal high roads of Germany. The weight annually carried on this little railroad amounts to nearly 2,000 tons; about one-tenth of which consists of the manufactured article.

About 200 laborers are constantly employed in this establishment, and it is greatly to the credit of the proprietors, Zeitoner & Heyne, that they have established a savings bank, a sick fund, and a fund for the support of widows and orphans.

We have spoken of the remarkable fall in the price of ultramarine. Competition and improved machinery and modes of operating have effected so much in that respect, that the whole price of the best article at the present time does not exceed that paid for the mere grinding, only eighteen years ago. This continual fall of price necessarily compels a corresponding expansion of the manufacture and sale to compensate for the deficit in profit. On this account scarcely a year passes without the addition of new buildings to this vast establishment. Considerably more than 5,000 tons are manufactured here yearly, at the average cost of from 25 to 37 cents per pound. The cheapness and exceeding beauty of the color cause it to be profitably and largely exported to France, in spite of the absurdly heavy import duty levied upon it there.

What we have said of this single manufactory, vast as it is, gives but a very inadequate idea of the extent and importance of the ultramarine manufacture in Germany. At the Industrial Exhibition at Munich no fewer than seven extensive manufacturers received medals, and two were honorably mentioned.

At the Parisian exhibition the French manufacturers did not dispute the excellence of the German ultramarine, or the exquisite beauty of its colors. But

they complained that it was of so many different shades that the various trades and professions which use the article were unnecessarily embarassed in the choice among so many gradations of color; for while the German manufacturers exhibited twenty different tints, the French furnished but eight. To this Germans replied, that in supplying so many different shades of the color they but complied with the public wish. Yet, nothing can be plainer than that in everything relative to color and cognate matters of taste, it is not the public which prescribes, but the artist, who elicits the artificial want. It was Phidias who created the Athenian taste for the surpassing beauty of the Phidian sculpture.

Up to 1849, France had only two manufactories of ultramarine. In that year a third was added, in Alsace, (Zuber & Co., at Rixheim,) which deserves mention with that of Guimet, who still sustains his long-established reputation. To such an extent is the manufacture of paper-hangings carried on at Rixheim, that the manufacture of colors might appear a merely accessory and subordinate branch. But such is not the fact; for, besides supplying the home demand, that factory exports to a very large amount. As long ago as 1849, the establishment employed 500 laborers, at an expense of $43,333. And the motive power consisted of 44 machines, exerting, in the aggregate, a sixty.two horse power.

As mentioned above, Gmelin's mode of using only the very purest raw material has been abandoned. But, though the ultramarine manufactured on his method was undoubtedly more costly, it is no less certain that it was also far superior in color to all other sorts of ultramarine. Economical manufacture is now sought in a variety of ways. A white German clay found in many parts of the country, and known to the trade under the name of "lanzin," is the most commonly made use of. White porcelain earth is preferable on account of its greater purity. A small portion of magnesia and lime is of no consequence; but if iron be present in greater proportion than one per cent., the utmost care is requisite to produce an even tolerable color. All foreign matters of a tangible kind are carefully removed by repeated washings; the clay is then dried, made red-hot, and reduced to a fine powder in a mill or stamper. These preparatory processes, simple as they appear to be, are in reality of great importance, and the mechanical contrivances for rendering them perfectly effective are among the most ingenious, as well as the most costly, of all the machinery employed in the manufacture. To the ground or crushed mass there are now added sulphuric natron, soda, sulphur, and coal or charcoal; the whole having been previously reduced to the finest possible powder. If coal instead of charcoal be used, such must be selected as after combustion leaves the smallest quantity of ashes. Sometimes rosin is used instead of coal, and, being decomposed by the heat, answers all the purpose of the mineral. Heated together, these various materials become fused into one mass. Upon the process thus far described, and, especially upon the exactly proper proportion of each of the materials, the result greatly depends.

With regard to one point in the procedure there is a wide difference between the French and the German manufacture. In the latter, Glauber salt or a mixture of that salt and natron is always used; in the former, only soda. The German mode is the more economical, because the sulphuric acetical natron is by the agency of the coal converted into sulphuric natrium, and thus the sulphur can be wholly or partially dispensed with if soda be added at the same time. It is true that a somewbat greater quantity of coal will be required, but there can be no comparison between its price and that of sulphur. As to the result, it does not seem that the one or the other method is very greatly preferable.

There is great difference in the proportions of the several components of this mixture; but the following may serve as a general rule:

GERMAN METHODS.

		Parts.
I.	White potter's clay, free from water	100
	Glauber salt, free from water	85 to 100
	Coal	17
II.	White potter's clay, free from water	100
	Glauber salt, free from water	41
	Soda, free from water	41
	Sulphur	13
	Coal	17

FRENCH METHOD.

White potter's clay, free from water	100
Soda, free from water	100
Sulphur	60
Coal	11

The next operation to be performed is that of what is called the over-glowing of this mixture. It is placed in melting pots of potter's clay, formed to withstand intense heat, and slowly dried till burnt. Absolute exclusion of air being indispensable, it is especially requisite that the melting pots be so tempered that they will neither burst nor become softened in the intense heat requisite to burn the mass within them. They may vary from 4 to 12 inches in height, with the like diameter. When filled up they are packed one on the other in a furnace resembling in form a flattened brickkiln. They occupy the whole centre of the surface, while the space on each side of them is used for the burning of similar pots. The furnace being properly filled, the mouth is walled up, and the firing commences. The burning continues during from seven to eight hours up to three days, according to the size, construction, and contents of the furnace. Fuel must be added till the mass is thoroughly incorporated and begins to melt. Upon this operation everything depends. If it be not properly conducted, the best and most accurately proportioned raw material will not yield a profitable result. The temperature must be of a certain height, which is to be ascertained beforehand by trials in a small testing oven. It approaches a bright red or incipient white heat, and must be kept at the same point during a specified time; and it must be made to heat the whole mass as thoroughly as possible. When the furnace is cooled, the glowed mass is taken out and cooled with water, and then repeatedly washed and drained to remove any salt still remaining. The now dried and spongy mass is next removed to the mill and broken and pulverized to the utmost possible degree of fineness; the powder is repeatedly washed with water, and after being thoroughly dried, again ground and nicely sifted. It has now reached the first stage of ultramarine, or what is called green ultramarine, and is ready either for sale or for transmutation into the blue colored or proper ultramarine. Hitherto, however, the green ultramarine has been in no very great request, as compared with the blue. It varies through several shades, from apple green to blue green; and in beauty it is far excelled by the copper color and even by the cobalt. Its chief, if not its only recommendations are its cheapness and its innoxiousness; and those qualities, important as they undoubtedly are, seem insufficient to counterbalance its want of brilliancy.

The next important operation is the transmutation of the green into the blue color. Here there is but one cause for anxiety. To obtain a perfectly beautiful blue, we must previously have a perfectly beautiful green. The latter is roasted with sulphur, air being freely admitted during the process. It sometimes happens that the change of color takes place without any interference. The sulphuric natrium contained in the mass causes spontaneous ignition on the admis-

sion of air, and when it ceases to glow we have still sulphuric acid present, and the green color is thus self-changed into a beautiful blue.

As to this process also of transmuting the green color into blue the French and Germans have their peculiar methods. The Germans use small iron cylinders for roasting; the French small hearth ovens, into which, however, the flame cannot enter. Hitherto cylinders of potter's clay have not been adopted, though we doubt not that they would serve just as well, and be even more durable. The cylinder being filled with from twenty-five to thirty pounds of green ultramarine, a vane (Flügelwelle) is set in motion so that the contents of the cylinder may not be burnt without being first thoroughly roasted within. A pound of sulphur is now passed through an upper opening into the cylinder, and while the wind-vane continues in motion the sulphur is gradually consumed. The addition of sulphur may be continued as long as the color improves in purity and brilliancy, but care must be taken not to continue it too long. After the color has been thus roasted it must once more be washed, dried, ground, and sifted.

The French method of roasting possesses this advantage, that, by allowing a freer accession of air, the green mass is the more speedily transmuted into blue. But, on either the French or German method, a large quantity of sulphuric acid escapes, which renders the factory a nuisance to its neighbors, while, were that quantity of sulphuric acid preserved, it would suffice for the production of all the Glauber salt used in the manufacture.

The quality of the green color is the rule and test of that of the blue; but something of its intensity also depends upon the manner in which it is ground—the finer it is powdered the brighter and clearer it becomes. But not all kinds are of equal beauty; lighter tones of color are frequently obtained without there having been any appreciable difference in the mixture of the raw materials. From these lighter and darker shades a medium kind is obtained by mixing them together, and adding other white or light materials. Where this admixture is resorted to, equal tones of color are out of the question; the shades vary from the softest sky blue to a glowing, almost ruddy, dark blue—the former generally forming a more compact powder, the latter a more loose and smooth one.

The principle upon which the blue color of the ultramarine is dependent is as undecided now as it was in the time of Gmelin. It is much to be regretted that his analysis of the Lapis lazuli, which so much conduced towards the manufacture of artificial blue ultramarine, has not been repeated and followed up. The foundation which he laid in scientific experiment has been built upon only in the way of the merest empiricism; and the success which has thus, in a merely monetary point of view, been obtained by the manufactures, has led not a few of them to imagine—how vainly we need not say—that henceforth they are quite independent of science. They forget that practical men, however ably they may profit by what science has taught them, do literally nothing towards clearing up what science itself has yet to learn. It was the science of Gmelin which alone laid the foundation for the manufacture of ultramarine as it at present exists; but who shall pretend to limit the improvements that might be made in that manufacture could another Gmelin arise to discover the principle on which the coloring of the ultramarine depends? Attempts have, indeed, often been made to lift the veil from this mystery, but hitherto they have been so made that it was impossible for them to succeed. Analysis has followed analysis, regardless of the fact that the ultramarine trade is not a preparation of determinate composition from which uniform results can be obtained. However accurately the operator may have treated the clay with water and sulphur, does not the color imbibe some portion of silicious matter? Nay, has not each specimen of clay different elements and different proportions of elements in its own composition? How are we to tell, even

from the most skilful and laborious analysis, which is the essential product and which the accidental? Which the portion which conduces to the production of the color, and which the portion that, to a greater or less extent, limits its quantity and diminishes its brilliancy? The time spent in analyses, thus inevitably indecisive, may be considered as completely thrown away. In truth, those analyses have rather raised questions than settled them. The influence of iron, for instance, upon the production and the color, which long ago was considered a settled fact, is now relegated into the realm of doubt. There seems good reason to believe that sulphur has a chief, if not the sole, part in coloring the green and blue ultramarine; but how—through what combinations? The material itself opposes difficulties to our clear view of the subject; and the difficulties are increased by the coincidence of two chemical processes, and by the facile decomposition of the material the moment it is attacked by reagents. Finally, we are but too imperfectly acquainted with the affinities of sulphur and the recently discovered sulphuric acids for the alkalies. This powerlessness of analysis to pronounce definite judgment has necessarily given rise to various opinions, founded not upon facts, but upon fancies, and, as usual in such cases, the opinion founded upon fancy has been more peremptorily asserted than the knowledge founded upon fact. Of the green ultramarine, we have seen it positively asserted, though without even an attempt at proof, that it is a simple combination of sulphur natrium, while another disputant is not less positive that it is a mixture of blue ultramarine with some yellow substance, the elimination of which turns the green to blue. Others assert that the acid has transformed the sulphur natrium of the green ultramarine into a sulphuric metal, combined with other and unascertained matter; that oxydization has taken place, and the sulphur has united with the undecomposed sulphuric natrium. According to others the oxygen acts upon the sulphur and forms a sub-sulphuric acid, or some other of the recently discovered sulphuric acids. In short there has been much disputation, but no approach to a conclusion which can be relied upon. To arrive at such a conclusion we must, as our starting point, first study the affinities of aluminum and sulphuric natrium.

All that we are thus far warranted in saying is simply this, that ultramarine contains silicious earth, potter's clay, natron, and sulphur. But, what else? *That* is the real question at issue. The silicious earth is, if not superfluous, at least inoperative, as regards the production of the blue color; but, though not itself the cause of the blue color, it at least supplies the fire-proof quality. Too much of it, undoubtedly, is injurious to the color. If the silicious acid be not fixed by natron, the blue color is either very much faded, or wholly destroyed, and the ultramarine is rendered unfit for the purposes of the porcelain painter. The artificial ultramarine has still another great advantage over the natural. While the latter could only be used for oil paintings, the former can be used in every art in which the blue color is indispensable, and consequently it has, to a very considerable extent, supplanted cobalt, litmus, and Prussian blue. Even when the ultramarine commanded a far higher price than smalt, (the latter selling, in France, at 47 to 50 cents per pound, while the former could not be purchased for less than from $1 62 to $1 73,) it was found that ultramarine was the cheaper article, for the simple reason that one pound of ultramarine would do the work of ten pounds of cobalt blue.

Ultramarine is a reliable color for oil painting or for painting on glass, for tapestry, and for paper-hangings in patterns, and for the coloring of soaps, candles, &c., &c., and it is not easy to over-estimate its importance in printing on wool, cotton, linen, or silk. To the French manufacturer, Blondin, belongs the credit of having been the first to use ultramarine in cotton printing. For six years he kept his application a secret; but, in 1844, Dolfus, a cotton manufacturer from Alsace, visited the French exhibition and made himself master of the process. Since then, as is said in the report of the French exhibition of

1849, "Guimet's method has travelled round the world, supplanting all the blue colors which had been previously employed by the cotton-printer." It must be confessed, however, that this statement is not quite exact, for the manufacturers still experienced some difficulty in using ultramarine. At first the color was, for the most part, not sufficiently fine, and consequently it affected both the spreading-knife and the rollers. That difficulty was obviated by the use of albumen, (the whites of eggs,) which thus became a by no means unimportant article of trade. It is used to condense, and to aid in spreading, the color, but requires some slight admixture of oil to prevent the decomposition which the albumen, pure and simple, was found to produce.

Ultramarine is used not only to produce a blue, but also a white. Every housewife well knows that blue of some kind must be used to counteract that yellowish tinge which linen and cotton goods acquire when washed. This use of the blue color is familiarly called *using the blue-bag*, but using the whitening bag would, in truth, be the more appropriate phrase. As a general thing the blue-bag is used far too freely. The effect should not be, as it generally is, to leave a blue tinge, but only to neutralize that yellow tinge with which we unavoidably associate the idea of imperfect cleansing. Ultramarine is also of important service in restoring linen and cotton yarns and fabrics to good color—from two to three pounds of the color sufficing to restore fifty pieces of linen. From ten to fifteen ounces are sufficient for the perfect bleaching of twenty pounds of yarn, and so effective is it in small quantities, and therefore so cheap, that even whitewashers use it to give increased brightness and cleanness to their white.

It was formerly considered, on toxicological grounds, that the use of ultramarine in whitening sugar was objectionable. We need here only so far advert to the discussions of the public journals upon that point as to say that two pounds and a half of ultramarine suffice to bleach fifty tons of sugar, being just $\frac{1}{50}$ grain to the pound, a proportion in which even that deadly corrosive, arsenic, would be entirely innocuous. Whether the sulphuric-hydrogen gas, which is liberated by the contact of the ultramarine in the sugar with the acid in wine, be offensive, is a question which we leave to the olfactories of the chemist to decide. How far ultramarine is, or may be, adulterated, chemists, we believe, have not, as yet, determined. Manufacturers maintain that it is not merely right, but even necessary, to mix potter's clay and gypsum with ultramarine, in order to get a lighter color; and to us it seems that, on that point at least, the manufacturer is a better judge than the chemist. The purchaser well knows that such admixture is made, and for what purpose, so that, whether right or wrong, there is, at all events, no deception; but if he wishes no such admixture in the ultramarine which he purchases, a simple and facile test of the quality is at hand. Adulteration is present if the color be not entirely discharged by strong acids, or if it change color when boiled in a ley of potash. The adulteration in this latter case has been made by organic matters, for the purpose of producing the fiery brilliancy of the natural ultramarine. If, to be thus tested, the ley assume a greenish tinge, the ultramarine contains a superfluous amount of sulphur natrium; and if the ultramarine adheres in hard clots or lumps the salts have not been sufficiently washed from it. When mere appearance is alone relied on as a criterion, the judgment, however practiced, is liable to be mistaken, for there is no other color which affords so much scope for visual deception.

There are two qualities to be regarded in the genuine ultramarine—the coloring and the covering quality—which maintain no direct ratio one to the other.

The coloring quality may be tested by mixing one part of ultramarine with ten parts of any white color—white lead, for instance, or clay, or gypsum—and then closely observing the tone of the mixture. These trials should never be omitted by purchasers, for in two ultramarines, which to the sight appear

exactly alike, there may be a difference, in both brilliancy and durability, of from one to two hundred per cent. Another important question is this: How much mordant does the particular ultramarine require? Nor is this important only in those great factories where the mordants are a considerable item of expense, for the artist also should be aware that every addition of mordant diminishes the clearness of the color. The less mordant the finer color, and *vice versa*.

It admits of no doubt that from remote antiquity the art of coloring of the raiment with which man invested himself had acquired a certain degree of proficiency. Pliny, though he gives no particulars of the processes, yet assures as that the ancients were well acquainted with the use of mordants, by which fixity is given to colors which otherwise would gradually change by successive gradations, or disappear altogether from the dyed fabric. Of those mordants he mentions human urine, ammonia, and certain salts, including rock-salt and soda, as serving to give at once brilliancy and fixity of color to spun and woven stuffs. And in another passage he intimates a still more advanced knowledge of the art of dyeing as practiced by the ancients. "In Egypt," he says, "cloths are dyed in a quite peculiar manner. The cloth is first thoroughly cleansed and then successively dipped into one or more solutions, and finally into the fluid color for which the previously used solution has so great an affinity that the cloth is dyed as permanently as instantaneously. What is most remarkable about this process is the fact, that though the dye-vat contains dye of only one color, the web of cloth is dyed of one, two, or several colors, according to the kind of solutions used for the preliminary washings or dippings. And further, not only is the cloth so permanently dyed that the color cannot be washed out, but the cloth itself is rendered stronger and more durable."

This language of Pliny shows, that our knowledge of the uses and effects of various mordants to heighten and fix color, and rather to improve than to injure the fabric of the stuff to be dyed, though doubtless much indebted to modern chemistry, is, substantially, as old as chemistry itself. In the case of ancient Egypt, such a knowledge need scarcely excite our surprise, that antique and mysterious land having been the source of the chemical science of at least all the people of antiquity. As nature herself suggested colored ornamentation, and the fugitive qualities of the earlier dyestuffs forced chemistry into the discovery of mordants, so the lack of a cultivated taste made the glaring scarlet and tawdry yellow the favorites of the earlier ages; just as, in our day, the same lack or imperfection of taste is apt to recommend those vivid hues to the favor of the childish and the unrefined. Next to the Egyptians the people of ancient India evinced most skill in the art of coloring. Job speaks with great admiration of the brilliant colors of Indian cloths. There is at this day in the museum of the *Industrial Society* at Paris a large and valuable collection of Indian colored stuffs, together with the utensils by which they were prepared. These stuffs should be called painted rather than dyed; the absorbent and mordant fluids were first applied with a brush, and the desired colors then laid on; those portions which were to remain white were at the outset covered with wax, and the outlines of the pattern traced on the remainder. There is also at Paris a shawl, ten feet long and five feet wide, the handiwork of Indian princesses, and so elaborately as well as beautifully executed that it must have employed the skill and industry of more than one generation of the royal and dusky workwomen. But everything else in ancient dyeing was surpassed by the proverbially pre-eminent

TYRIAN PURPLE.

Inventions have their place in Mythology, and not improperly; for if chance plays no inconsiderable part in the inventions and discoveries of the present days, so, also, it did in the days of old. All have heard, or read, the story of the dog

which occasioned the discovery of the beautiful Tyrian purple. As Hercules (so runs the fable) walked one day on the sea-shore with the fair object of his love, her pet dog, playing around them, seized an open sea-snail, and dyed his mouth of so beautiful a color that the lady uttered a wish to have a dress of that self-same hue. Hercules, of course, succeeded in granting her desire. It is assumed that this discovery dates from the year 1500 B. C.

For nearly all that we know of purple dyeing we are indebted to Aristotle, Pliny, and Vitruvius. Pliny mentions two shell-fish that yield the "purple," the "buccinum," so called, on account of its resemblance to a trumpet, and the "purpura." The coloring substance was said to be contained in a transparent and branching vein at the back of the creature's neck, and while the animal was alive, the fluid had a mucous or creamy consistence. If the fish were small, they were pounded; but if large, containing so much as an ounce of the highly valued fluid, the vein was detached, its contents mixed with five or six times its weight of water, and to the mixture thus formed soda was added, in the proportion of twenty ounces to every hundred pounds. The whole was then put into lead or tin vessels and kept in a moderately warm place for five or six days, the scum being from time to time carefully removed. As soon as the fluid assumed the precise tone of color that was desired, the wool was dyed. The process was very simple. The wool, being thoroughly cleansed from grease and all other impurities, was plunged into the dye for some five or six hours, or even longer if the object was to double dye the material, (*dibaphes*,) in which case it was highly esteemed and proportionably high in price. Wool thus dyed commanded in the reign of the Emperor Augustus the enormous price of two hundred dollars per pound, nearly its weight in gold!

We learn from Vitruvius that various countries had their peculiar shades of purple. At the north, the shade approached to violet, while at the south it became the vivid red which we now term a bright scarlet. Pliny also distinguishes two different shades of purple—the tyrium or purpura, a dark crimson like that of coagulated blood; and the amethystinum, the light violet blue of the amethyst. Both authors agree in stating that an excellent purple was obtained from some plants; our own madder, it would seem, being among them. Madder (Rubia tinctorum) was undoubtedly known and cultivated in several ancient countries—Italy and Judea, for instance. Woad, too, (Isatis tinctosria,) was well known to the ancients, and served to give to the purple that fine violet tint which was so much prized.

The purple-yielding shell-fish were found on all the coasts of the Old World; and in Greece, Italy, Dalmatia, Istria, and Egypt, there were large dyeing houses. Of course they used up an immense number of these minute animals; but the supply was equal to the demand. For instance, Mount Testacco, near Tarentum, consisted almost entirely of the shells of the Murex brandaris, which we believe to be the shells from which the Roman dyers extracted their coloring matter. According to Tacitus, the Germans had a purple dye which was especially in request for linen. But above all the purple dyes of the ancients, that of Tyre and Sidon was admired, and it was a very important item in the commerce of the merchant princes of Tyre. No color has ever been so long valued and so profusely lauded as the purple. In the days of Moses it was the distinctive color of the great and the wealthy; Homer makes Æneas offer a superb purple robe to Bellerophen; Dives, in the New Testament, is "clothed in purple and fine linen;" and it was in a robe of purple that the stern Roman Imperator triumphantly returned to the seven-hilled city, after vanquishing and subjugating some far barbarian foe. Pliny speaks of "the Tyrian purple" as being a color so representative of dignity and majesty that Roman lictors made way for it with their fasces and their followers. Not only was it the distinctive mark for both young and old of high rank or great wealth, but was still further honored by being the indispensable color of the robes of those who reve-

rently sacrificed to the gods, to obtain their favor or to avert their wrath. Pliny is so much in love with the purple that he deems it no mere idle vanity, but a laudable and natural yearning in men eagerly to desire it.

To the great majority of Romans purple was forbidden for a long time by its enormous cost as compared with the moderate fortunes of most of the plebeians; but when wealth flowed into Rome and corrupted the Romans, purple was fast becoming the only wear, and the Cæsars, from Julius downwards, prohibited its use by private citizens under pain of death. The Byzantine emperors made it penal even to write with purple ink, the use of which they monopolized for their own imperial signatures; and the very art of dyeing in purple was confined as a privilege and a monoply to favored individuals. As a natural consequence, the art decayed, and at length was entirely lost towards the end of the twelfth century, though so recently as the preceding century the Greeks, Saracens, and Jews, had been renowned for their skill as dyers. During the twelfth century the purple was less various in its shades, and very much less in request. But though the fickle tyrant Fashion, for a time, discarded purple in favor of scarlet, procured from the Thermes, the traditionary reverence for the imperial purple was not extinct, for even to this day, throughout the Old World, "purple" is synonymous with imperial power and place.

Strangely enough, while purple-dyeing was a disused, if not a forgotten, art in many of the countries to which it had once procured so much profit, it still continued to be considerably practiced in Britain. With that island the ancient Phœnicians are known to have had considerable commerce, the Britons, as we learn from Herodotus, supplying the Phœnicians with tin, and it is probable that it was from the Phœnicians that the Britons learned the art of purple-dyeing. The practice of the art existed in England till the close of the fourteenth century; and so late even as 1684 an Irishman is said to have made a large fortune by the peculiar skill with which he gave the purple dye to fine linen and other articles of female apparel. He, like the ancients, obtained his dye from a shell-fish.

The Chinese are said to have had a dye resembling the purple; and in the New World, according to Don Antonio d'Ulloe, the people of the provinces of Guayaquil and Guatemala were, from the earliest times, possessed of a beautiful red color, which they obtained from certain sea-snails of a size not greater than a hazelnut. These, on account of their scarcity, were highly prized, and were used only for dyeing choice and costly matters, such as beads, fringes, braidings, &c. It was the popular belief that both the weight of the animal and the color of its juices varied with the hours of the day.

The purple dye had at length become so entirely forgotten that what the ancient writers had said of it was regarded as a fable, invented by the Phœnicians to conceal their knowledge of the cochineal insect. A shell-fish yielding such a fluid was no longer known. It was not until the seventeenth century that the first attempt was made to red'scover and to ultilize the long-forgotten secret of antiquity. Then, indeed, men were enabled once more to view the prodigy with their own eyes, for in the West Indies, in Peru, on the coasts of Italy, France, and England, there were found muscles whose vital juices, from being at first colorless, soon took, successively, the shades of yellow, green, blue, and finally a splendid purple. William Cole, of Bristol, in England, was the first who, in the seventeenth century, experimented for the revival of the lost art of dyeing in purple, and he used only the common muscle which is so abundant on the shores of England, and after long trial at length discovered the long-sought-for shell-fish in the *Purpura Lapillus.* "If," says he, "we carefully break the shell we find, near the head of this shell-fish, a white vein lying in a furrow, and within that vein is a white, creamy, and somewhat glutinous fluid, which is the much-desired dyestuff." His description precisely coincides with that of Aristotle and of Pliny.

In 1709 Jussien made similar researches on the French coast—researches which, in the following year, were continued by Reaumur, who delighted to make theory and speculation the obedient handmaidens of every day utility. Somewhat later the soft-shelled molluses of the Mediterranean shores were carefully examined by Italian naturalists, and so well has their pains-taking example been followed up that we are now acquainted with a goodly number of molluses that yield the purple dyestuff. For the most part they belong to the families of the *Murex* and the *Buccinum*, of Linnæus; and it is thought that the *Murex trunculus*, of Linnæus, (one of the most abounding of the Mediterranean sea-snails,) and the *Purpura* and *Purpura putula*, of Lamarck, are identical with Pliny's *Buccinum*. The *Purpura Lapillus* is quite common on the European shores, and is believed to have been the most important among the purple sea-snails of antiquity. Lesson thinks that the *Janthina fragilis* is the true buccinum of antiquity. It is a native of the Mediterranean. In stormy weather it is thrown upon the coast of the French department of Aude in such vast numbers as actually to cover the strand. Lesson attributes to Narbonne (the *Narbo Martins* of the ancients) great skill and celebrity in the art of purple dyeing in the times of ancient Rome. Other writers say that though the Garlish purple was very splendid, it yet was very evanescent. The janthina undoubtedly affords a bright and beautiful purple, and when taken out of the water yields the fluid to the average amount of about an ounce. But the fluid is furnished by a gland entirely different from that spoken of by the old writers, a fact which it is difficult to reconcile with the ancient statement. Moreover, the modern purple is very evanescent, while the ancient was valued no less for its durability than for its beauty. Thus, in Plutarch's Life of Alexander the Great, we read that the Greeks found in the treasury of Darius purple stuffs to the value of five thousand talents, and that, though some of them were nearly two centuries old, the color had not at all faded. Lesson says that the coloring fluid yielded by the janthina passes through the same changes of light and shade as the vegetable colors do. With alkalies, it becomes blue; with acids, red.

Some writers include *Aplysia depulans* and *Scalaria clathrus* among the purple sea-snails, but this is doubtful. It is true that the aplysia sometimes voluntarily, always when alarmed, does emit a beautiful purple fluid, and, in the latter case, in such quantities as to color the water for several yards around. Probably the purple fluid, in the case of the shell-fish, is analogous to the ink of the cuttlefish, the concealing and protecting provision of the otherwise defenceless creature. The fluid is colored at the moment of its ejection, but the tint is of slight duration. The fluid of the *Scalaria clathrus* is still more evanescent—time and exposure to light discharging it entirely. Of the *Planorbis corneus* Wallis says: "If you put salt, ginger, or pepper into its mouth it yields a purple fluid, but the color is so evanescent that we know of no mordant that can fix it."

At present we are acquainted with a great number of purple-yielding shell-fish, but we cannot identify any of them with the purple sea-snails of the ancients, the descriptions left us by the old writers being too general and vague.

In our own time Bancroft has industriously experimented with the dyeing fluid of purple sea-snails, and he asserts that they yield a fluid which surpasses everything else in animal nature, alike for the brilliancy and the permanency of its purple, and for the facility and simplicity of its use. Within the fish, or when separated from it, the fluid has a creamy appearance, or, as Reaumur phrases it, resembles a well-developed pus. The textures to which it is applied become first of a light, then of a darker green, next blue, and acquire finally a rich deep purple tint, inclining to crimson. According to Bancroft, the gradual prismatic changes of the colors are as beautiful as they are remarkable. Even the most powerful chemical agencies, whether mineral acids or the most corrosive alkali, can only subject this purple to one change—wash the fabric in

strong soapsuds and the purple becomes a magnificent and permanent crimson. Dyed with this singular fluid the fabric passes through all the prismatic changes of color of which we have spoken, in a very few minutes; and if exposed to heat as well as light, the changes are so rapidly effected that the eye can scarcely appreciate the passing of one hue into another; but if, on the contrary, light be completely excluded, the first pale, yellowish green will remain unchanged for years. Bancroft proved this with some linen thus dyed, and kept for nine years in the dark.

As to the causes of the changes of color they are not clearly understood. Berthollet thinks that the coloring matter absorbs oxygen. Bancroft attributes the effect to light. He justifies his opinion by reference to the coloring of prints, flowers, &c., which coloring is known to take place, not from warmth, but from light. It is by the mere exclusion of light that we bleach, for instance, endive and celery.

As far as we are at present informed, the chemical nature of this coloring matter is as little known as the *modus operandi* of its successive changes after being applied to a textile material. Highly as Bancroft and others have praised the purple, it has had its day of popular favor. For dyeing fine muslins, and as a marking fluid, purple is still occasionally used. Even as long ago as the thirteenth century, scarlet, from Kermes, instead of purple, was the adopted color of the Hungarian magnates. Our numerous dyestuffs, and our facile and economical dyeing processes, render us independent of the ancient purple.

With the revival of science and art from the decadence into which they had sunk, during what are not unjustly called the dark ages, dyeing, like other arts, started into new, vigorous life. Till the fifteenth century, and still later, Italy bore away the palm in the art of dyeing, for which Florence and Venice were especially renowned. The discovery of America gave a great impulse to the same art, dyestuffs being furnished which were entirely unknown to the Flora and the Fauna of the Old World. From the Italians the mastery in the art passed to the Flemings; and when the religious persecutions by Spain drove the Flemings into exile, these latter carried their art into France and England. It was a native of the Netherlands, Cornelius Drebbel, by whom, in 1650, the discovery was made that cochineal was capable of yielding a dye far surpassing in beauty the purple of the ancients. Drebbel was at work in his laboratory, when an accident having thrown some *aqua rega* over the tin fastenings of the window panes, and thence into a bottle full of an aqueous infusion of cochineal, the latter on the instant assumed that magnificent scarlet tint which is now so well known. Drebbel was too acute and too reflecting an observer to neglect such an indication, and from that time cochineal has played an important part in the art of dyeing.

Modern chemistry, however, has done more for this art in single years than had previously been accomplished in centuries. Pure and effective mordants and mineral colors have wonderfully changed both the laborious and the economical processes of the art. Colored garments were formerly the external sign of rank or opulence. At the present day, thanks to the labors of men of science, the man who wears the homeliest and cheapest garb, as to quality of fabric, may yet wear it of the most tasteful color. Chemistry, however, is still making and will long continue to make still further improvements in this art, as in others. One of the latest acquisitions thus made by the secluded men of the laboratory is that of the much-valued coloring material known as

MUREXINE, (MUREXIDROTH.)

This color is extracted from the uretric acid contained in urine.

The ancient adepts, or alchemists, carefully analysed that fluid, in which indeed they sought their arcanum, and in the course of their experimenting they

produced volatile alkali, and phosphoretic ammoniæ natron. The last named salt was probably known to the ancients, and used by them in soldering metals.

The uric acid which now is so importantly utilized in urine, or rather in uric calculi, was found by Scheele, in the year 1776, which are, for the most part, composed of that acid, itself a component part of the urine of all carnivorous animals, and perhaps of all animals having a renal secretion;* being in the excrement of birds, snakes, and even in that of caterpillars, snails, &c.

Scheele remarked, that a solution of uric acid in acid of saltpetre left, when evaporated, a red sediment, and would stain the skin a fine red color. In 1818, Prout, by the action of ammoniæ on a solution of uretric matter in acid of saltpetre, discovered a material which he called "purple acetic ammoniæum," on account of its splendid color. He thus describes the process of producing it: Pure acetic acid and acid of saltpetre are mixed with an equal volume of water and gently warmed till solution takes place and strong fermentation is produced. The superfluous acid of saltpetre is then diluted with ammoniæum and the whole reduced by evaporation. During the operation the color gradually changes from purple to red, and through numerous shades of dark red. Greenish granular crystals are precipitated, which consist of purpuric acid and ammoniæum. This reaction is remarkable, and so positive that chemists have long availed themselves of it to detect the presence of uretric acid in any organic substance, for this characteristic coloring only takes place in the way mentioned and where uretric acid is present.

Liebig and Woehler, in 1837, also produced this brilliant colored matter while experimenting on the changes of uretric acid under the influence of oxydating matter. It appeared in the form of small crystals, or short four-sided prisms, which when held up to the sunlight appear of a rich garnet-red color, changing, under a reflected light, to a greenish metallic splendor not unlike that of the wing of a rose-chaffer. Those chemists believing Prout to be mistaken as to the chemical composition of this beautiful colored material gave it the name of "murexid," from *murex,* the purple snail. It is not formed directly from the uretric acid, which is first converted into aloxan and aloxantine by means of acetic saltpetre. These are two colorless combinations of little durability, but, acted upon by ammoniæum, they exhibit the purple-red coloring.

Prout produced several compositions from purpuric acid and other bases, such as lime, quicksilver, and oxyde of zinc, and all such compositions were remarkably beautiful in color. He also claimed that some of those compositions can be utilized not only in painting, but also in the dyeing of wool and other textiles, but his statement could not immediately be acted upon. In the first place, his description was so vague and general that experiments often failed when based upon it. Then the temperature, not less than the concentration of the fluid, is of great importance in producing the result, which often is very different even when the accurate prescription of Liebig and Woehler is followed. Moreover, in Prout's time the raw material was insufficient for the production of a large and constant supply of this dyestuff. It is true that, as we have stated, the uretric acid is furnished by many species of animals, but it is furnished only in very small quantities. Man, for instance, secretes only about one-third of a drachm of it in twenty-four hours. The excrement of birds is distinguished for its great proportion of uretric acid; it is $\frac{1}{72}$ part of the weight of dried pigeon's dung. But that could not be produced in large quantities any more than the excrement of snakes, which consists chiefly of uretric-acidical salts.

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* Millions of dollars are annually paid for guano by the farmers on both sides of the Atlantic, yet they, for the most part, suffer the urine of their live stock to sink uselessly into the ground or to pollute and empoison the air, forgetting, if they ever knew, that guano is only more valuable than the manure of the farm-yard or the stable because birds have no urinary passage, and therefore their fæcal excrement contains all the uretric salts.—*Translator.*

If even the chemists were insufficiently supplied with the raw material, still less could it be procured for the purposes of industry. In Prout's time, the cost of a pound of uretric acid was from thirty-two dollars to forty-two dollars and forty cents; it can now be bought for from two dollars to two dollars and fourteen cents, though it is not in a chemically pure state. This great reduction, which enables the manufacturer and artisan to be plentifully supplied with murexid, is owing to the introduction of

GUANO.

This substance is imported from Peru into various parts of North America and Europe, at the rate of between one and two hundred thousand tons per annum. Guano is found in vast quantities in Peru and on many of the cliffs and islands in that part of America between the 13th and 21st degrees of south latitude. It is the excrement of sea-birds, and contains as much as four per cent. of uretric acid. In those regions the sandy soil could be but unprofitably cultivated without the aid of guano. It is known that as early as the twelfth century manuring with guano was practiced there. Under the Incas, guano was considered so valuable that killing the young birds on the guano islands was punishable by death.

Each of those islands had its superintendent, and each island was assigned to a particular province. From 6,000 to 7,000 tons were annually used in Peru alone; and when Alexander von Humboldt was exploring America, there were as many as fifty small coasting vessels employed exclusively in the transport of guano. Humboldt took some samples to Europe, where they were analyzed by Klaproth, Fourcray, and Vauquelin. The celebrated traveller and writer also published what he had learned as to the importance of guano to agriculture, but for some time his words remained unheeded. In Germany, Liebig's call upon the cultivators of the soil failed to stir them into activity, and it is even now insufficiently used, even by England, whose severely worked land more than almost any in Europe requires such a return of the elements of which years of grain-growing have deprived it.

Liebig and Woehler were the first chemists to experiment on guano. In the course of their inquiries on the subject of uretric acid they had often been embarrassed by want of material; they therefore requested William Kind, an apothecary of Bremen, to procure them some, and in due season received a hundred pounds weight from Valparaiso. So much have agriculturists been enlightened since that time, that, in several European countries, guano is an article of considerable yearly importation, and the raw material of uretric acid is never wanting. And such is the potency of modern chemistry that guano, so highly offensive to the nostrils in its raw state, is made to yield some of the most delicate of the perfumes which are used by the fair and the fashionable. A greater contrast than that presented in this case by the raw material and the article it is compelled to yield can scarcely be imagined.

The first attempts to render the murexid available for dyeing purposes were made by Sacc, in Alsace, that high school of the art of dyeing; and he succeeded in giving to wool an amaranth color far more beautiful than that obtained from cochineal. This induced Schunberger to try a new course of experiments, in which, if he did not entirely succeed, he at least ascertained that white textures could be thus dyed both handsomely and durably. Sacc maintained on this occasion that the coloring matter of the cochineal, the kermes, &c., has some connexion with murexid. He claimed to have discovered that birds, and especially those of brilliant plumage, the parrots, for instance, while they are moulting, secrete scarcely a distinguishable trace of uretric acid, but secrete a considerable quantity as soon as they recover their full plumage. What, then, becomes of the uretric acid when it is no longer excreted from the body?

May it not be metamorphosed into some other substance which, like the alloxan, is capable of dyeing the feathers? These questions are only suggested, and we are not as yet able to supply the answers; but this hypothesis, if adopted with regard to birds, must also be extended to reptiles, insects, &c.

The murexid is now a favorite dyeing material, strongly competing even with cochineal. Germany, as usual, was the last to adopt it. In a new and little known process mistakes are quite natural. When this new dyestuff first made its appearance as an article of trade, under the names of purple carmine, purple murexide, or paste murexide, it was in the form of a dirty-brown pulp. Though it sold as high as $4 80 to $6 the pound, it was a very inferior quality, and in many cases contained not more than from four to five per cent. of the murexid. Of course, this inferiority arose from imperfect preparation.

To extract uretric acid from guano, the latter must be moistened with diluted acid of salt, and warmed. The calcareous salts and everything soluble in water or acids is removed, while the uretric acid, with a not inconsiderable quantity of sand and other adulterations, remains. The well-washed residue is then put, in small quantities, into acid of saltpetre of 1.45 specific gravity, and the vessel must be kept cold. Only when the fermentation subsides should more uretric acid be added. By this procedure alloxan and alloxantine are obtained. But it must not be forgotten, that, as it is impossible to hit upon the exactly correct quantity of the acid of saltpetre, we should always have a surplus of the acid at hand. It must be remembered, too, that very noxious fumes escape during the evaporation of the solution. The above-mentioned chemical products of uric acid suffer a further decomposition, and form combinations destitute of murexid. To avoid this, it is necessary that to the solution of uric acid and acid of saltpetre there should, during the evaporation, be an addition of ammonæum. Murexid may be formed without that addition, but always at the expense of the alloxan and alloxantine; for if the ammonæum be absent during the evaporation, the alloxan and alloxantine are required to supply its place in the chemical production of murexid during the evaporation; and, moreover, the decomposition just spoken of continues, and we run the risk of having the murexid destroyed as fast as formed.

The murexid must not be suffered to crystallize; the solution is to be evaporated only to the consistency of a pulp. In the whole process there should be the utmost care observed that only the purest and best murexid be produced. The high price of the pure article would be more than compensated by its greater efficacy in dyeing. All textile fabrics, silk, wool, cotton, and flax, may be dyed with murexid, which is also used in cotton printing. Truly splendid colors are obtained by using the oxmuriate of mercury as the adhesive medium. We are obliged, however, to confess with regret that the murexid red cannot compare with the ancient purple as to durability. Samples on which we experimented with the usual re-agents lost their colors, however beautiful. We do not speak of such re-agents as the corroding alkalies and potent mineral acids which would affect, and in our experiments did affect, black no less than murexid red. But this latter faded under the application of even weak vegetable acids, such as vinegar, lemon-juice, &c., and even perspiration left visible traces upon the delicate tincture. Here, no doubt, are considerable defects; but it is to be remembered that the whole art of dyeing with murexid is as yet in its infancy. Even the ancient purple was not indestructible, and in the present day the public demand is not for indestructibility, but for cheapness. If the color please the eye and the price per yard be low, little is thought about the durability of the article. Time and the progress of chemical science will doubtless remedy the defects spoken of, since there can be no question but that this color is susceptible of great improvement. If the murexid be precipitated from its solutions by metallic salts, as, for instance, oxymuriate of mercury, or salts of lead or zinc, very beautiful lac colors are obtained, which can be used for the paint-

ing or printing of paper-hangings; and quite a new field is opened to the dyer and printer of textile fabrics by the affinity of this coloring material for various metallic salts. Not only several beautiful shades of red can be produced with it, but also yellow, blue and violet.

And thus it is that our sober and utilitarian day steals one by one its glories from hoar antiquity. What the mightiest and haughtiest magnates of the olden day claimed as their exclusive privilege has now become common property to the humblest as well as to the highest. A striking proof, this common property in beautiful colors, of the superiority of the present age in its utilitarian tendencies to that antiquity which we so highly, and, in a purely æsthetic point of view, so justly, glorify. The animals which supplied the ancients with their costly purple are perfectly known to us and easily obtainable, but we cast them aside, because we can more readily obtain our objects by other means. Whether the murexid be the very "purple" of the ancients is a question fairly open to discussion; but that it is so is by no means improbable. We know that uric acid is a constituent of the common snail, and, it is not unreasonable to suppose, of the purple snail also, though the fact be not experimentally proved. Putrefied urine, added to the fluid of snails, furnishes ammonia; so that the ingredients for the formation of murexid are certainly present.

Should the murexid red be still supposed inferior to that resplendent purple which the old writers so eloquently extol, let it not be forgotten that the skill of the dyer was of old limited almost to that one really splendid color, and that our modern wealth of gorgeous colors and delicate tints was then not dreamed of. Could we place our murexid, however, side by side with the true Roman purple, the former probably would not lose by the comparison. The glories of antiquity, like the prestige of our modern great men, might lose not a little of their illusion were we placed in closer contact with them.

METHOD OF PRESERVING LEPIDOPTERA.

PREPARED FOR THE SMITHSONIAN INSTITUTION BY TITIAN R. PEALE.

THE difficulties in the preservation of zoölogical collections generally arise from two causes, namely, moisture and destructive insects.

To guard against the effects of moisture requires so little ingenuity that I shall merely allude incidentally to the necessity of drying the specimens well at first, and then keeping them in dry places.

The greatest of all difficulties to guard against, particularly in this country, is the voracity of the destructive insects belonging to the entomological families of *Dermestidæ* and *Tineidæ*. These are the worst enemies of the zoölogical curator, as well as the fur-trader and careful housewife.

Tinea tapetzella, the clothes moth, which troubles the housewife and the clothier, does not disturb the entomologist; consequently the whole of this family may here be passed by in silence.

Dermestes lardarius (the bacon beetle) and *Anthrenus musæorum* (museum beetle) and their congeners are the great depredators. In the time of the Pharaohs of Egypt they destroyed the mummies which were intended to last through all time, and now in our day they destroy the specimens with which we hope to enlighten posterity. As they have been known for centuries, numerous poisons and various devices have been resorted to in order to destroy them, but they remain as numerous as ever, being naturalized and abundantly propagated wherever man has made his resting-place on the earth.

In early life I was a devoted student of nature, an industrious collector of specimens, and a somewhat expert taxidermist. It is, however, needless to record the fact that I lost my specimens, like others, almost as fast as they were collected, and, as a last resource, I was compelled to undertake a careful study of the habits of the enemies with which I had to contend, in order to learn the means of subduing them. I early found that substances containing albumen or gelatine stand but little chance of escaping the ravages of the *Dermestidæ,* and must be destroyed, sooner or later, by their attacks, whether moist or dry, unless chemically changed in character, or kept by some mechanical arrangement beyond the reach of the insect. I say chemically altered, because, as in the case of gelatine soaked in corrosive sublimate, the coagulation of the material, which is a chemical change, so alters the matter as to render it no longer a proper food for the insect. The means of protecting, therefore, must be adapted to the kind of specimens to be preserved. Our present object is principally to describe a successful experiment in preserving *Lepidoptera,* and to this subject we shall chiefly confine our remarks.

The vapor of camphor, and the essential oils generally, are sickening or fatal to the perfect insects of the family *Dermestidæ,* but have little or no effect upon their eggs or larvæ; consequently, although these perfumes in close cases are useful to keep out the parent insects, they will not destroy the progeny after a lodgement has once been attained. The several species of this family, unlike most other insects, have no fixed period or season for depositing their eggs, and consequently require to be vigilantly guarded against at all times. They are about one year in attaining their full growth, in which time they cast their skins four or five times. Their feet, though armed with claws, are unfit to climb on a hard smooth substance like that of clean polished glass. They spin no silk, and therefore cannot, like many caterpillars, construct a fibrous ladder to climb

up the same surface. Upon these simple facts I based plans for the preservation of *Lepidoptera* as long ago as 1828, and since then no specimen which I have wished to preserve has been touched by *Dermestes.*

For collecting insects I have generally found the case described by LeValliant, during his travels in South Africa, the most convenient. This principally consists of a box filled with perpendicular slides covered with cork, to which the specimens are pinned, and a horizontal drawer at the bottom to receive any specimens which may be disengaged from the slides during transportation, and thus preventing it from damaging those which remain on the slide. The spaces between the slides being all open below, a single bag of camphor placed in the drawer will diffuse its vapor through all the compartments, and thus prevent the attack of ants, roaches, and other large insects which prey, especially in tropical countries, on the fresh specimens. By placing the specimens on the perpendicular slides, tile-fashion, I have found that double the number could be accommodated, while additional security was gained by this arrangement from the danger of the loosening of the pins by the jolting of the box.

In the preparation for the cabinet I begin by pinning the specimens to be preserved in the order in which they are finally to be preserved on the bottom of a shallow box, lined with a thin layer of cork, or, better, of balsa-wood, which is easier penetrated by the point of the pin. This box must be of precisely the same length and breadth as those which are to form the permanent cases of the cabinet. When the specimens have been arranged in the order to suit the taste, and so that one may not overlap the other, the box, with its contents, is transferred to an oven, which I also invented in 1828 for this special object; but which has since been used, generally by chemists and others, for a variety of purposes. It is surrounded and heated by boiling water, the temperature of which is sufficient to kill the eggs and the larvæ of the *Dermestes,* but is not sufficient to injure the specimens of butterflies, moths, &c. The specimens are kept in this oven several hours, or during the night. (See *Fig.* 1.)

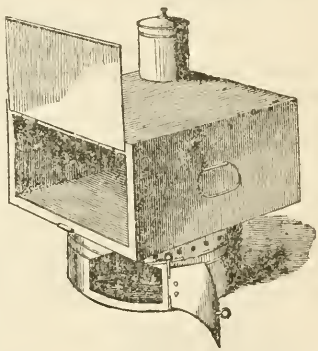

Fig. 1.

After the specimens have been sufficiently baked I lay a clean pane of plate glass immediately over the specimens, which, resting on the perpendicular sides of the box, does not touch them. On the upper side of this glass plate, face-down, and directly over the pin securing each specimen, I attach, with fish-glue, (isinglass,) a circular piece of paper, about a quarter of an inch in diameter, containing a printed number. The size of the glass plates which I use, and find most convenient, is eight and a half by ten and a half inches. It is commonly imported, and used for cheap mirrors. It must be cleaned with dilute nitric acid, or the surface will be liable to become foggy in damp changes of weather.

Next small cylinders of cork of the same diameter as the papers containing the numbers, and just large enough to support the specimens, are cemented to the glass plate directly on the top of each of the paper numbers. The cement used for this purpose is composed of about equal parts of resin, beeswax, and chrome green, melted, for convenience, over a nursery lamp placed on the table beside me. The pieces of cork are dipped into the composition, and while the portion of the latter which adheres is still liquid, they are attached to the glass in their proper positions.

The next operation is to attach the plate glass to a wooden frame, thus forming a shallow box, of which the glass plate will be the bottom, having the numbers and cork supports on the inner side. This frame is made of strips of

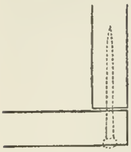

Fig. 2.

white pine, ready planed by the carpenter, to the dimensions of one inch and an eighth in width, and three sixteenths of an inch in thickness. These slips are cut to the proper length, and fastened in the form of a rectangle by common pins at the corners, as shown in *Fig.* 2. Previous, however, to forming the slips of wood into frames, they are coated on all sides with tinfoil, which is attached by means of the cement above described, omitting the coloring matter. I find it convenient to keep on hand while preparing these cases a supply of tinfoil, coated on one side ready, when required, to be cut into slips of the proper size. The coating of cement is put on by means of a brush dipped into the melted material.

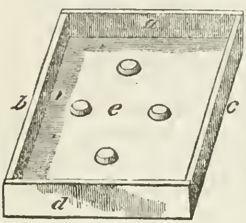

Fig. 3.

Another plan, and I believe the best one, is to have the cement enclosed in a muslin bag, which may be tied to the end of a short stick; the tinfoil is to be spread out on a hot iron plate, say the top of a stove, when the bag containing the cement is rubbed over its surface, and the heat being sufficient to melt the wax and resin, the foil may be evenly coated, and on removal from the hot metal plate, as it cools quickly, may then be rolled up and kept in readiness to be cut in suitable pieces for use.

Fig. 3 exhibits a box thus formed, of which *a*, *b*, *c*, *d* are the wooden sides covered with tinfoil, and *e* the glass plate, on the inside of which are placed the paper numbers covered by the cork supports. The next step in the process is to transfer the butterflies in the preliminary box to their several supports on the glass plate, and to securely pin them to the cork so as not to fall off in the ordinary handling of the cabinet. After this the box is to be permanently closed with a glass cover of the same dimensions as the one which forms the bottom, and the whole fastened air-tight by means of the tinfoil. By this arrangement the specimens are hermetically sealed between two parallel panes of plate glass, which allow the under as well as the upper surfaces of the insect to be seen, while the whole is preserved from atmospheric changes and the ravages of insects.

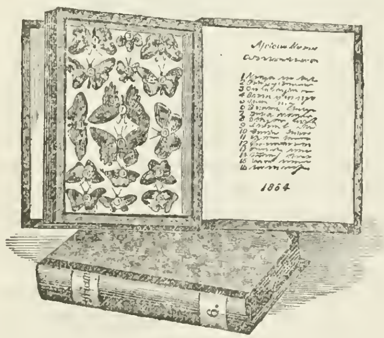

Fig. 4.

The cases containing the specimens are now furnished, so far as the means of preserving the contents are concerned, but this case itself requires to be guarded from injury and kept free of dust. For this purpose it is placed in an outer case, which I prefer to make in the form of a book with covers, which, on opening, exhibit the glass plates and the contents of the case. On the inner surfaces of these covers I write, or print, the names of the specimens therein contained.

All the cases which form the whole cabinet are arranged in an ordinary bookcase with glass doors, and when properly ornamented on the back resemble a series of large octavo volumes. The cases should always be kept like books in a case, in an upright position, and never allowed to lie on their sides, except when in use. The reason for this will be obvious: the perfect *Dermestes* might find a small hole in the tinfoil wherein to enter, or, deposit its eggs; but should the glass be upright, neither the old nor the young depredators would be able to climb up to the specimens, which they possibly might reach if the case should lie on its side.

Thirty-five years' experience with cases made as above described has proved the correctness of the theory of their construction.

AN ACCOUNT

OF

A REMARKABLE ACCUMULATION OF BATS.

BY M. FIGANIERRE É MORAO, MINISTER PLENIPOTENTIARY FROM PORTUGAL TO THE UNITED STATES.

In the winter of 1859, having purchased the property known as Seneca Point, in the margin of the Northeast river, near Charleston, in Cecil county, Maryland, we took possession of it in May of the next year. The dwelling is a brick structure, covered with slate, in the form of an ⊥, two storied, with garret, cellars, and a stone laundry and milk-house attached. Having been uninhabited for several years, it exhibited the appearance, with the exception of one or two rooms, of desolation and neglect, with damp, black walls, all quite unexpected, as it had been but very slightly examined, and was represented in good habitable condition, merely requiring some few repairs and a little painting.

The boxes, bundles, and other packages of furniture which had preceded us, laid scattered around and within the dwelling; these, with the exception of some mattresses and bedding for immediate use, were hastily arranged for unpacking and placing in order at leisure. The weather, which was beautiful, balmy and warm, invited us towards evening to out-door enjoyment and rest, after a fatiguing day of travel and active labor; but chairs, settees, and benches were scarcely occupied by us on the piazza and lawn, when, to our amazement and the horror of the female portion of our party, small black bats made their appearance in immense numbers, flickering around the premises, rushing in and out of doors and through opened windows, almost obscuring the early twilight, and causing a general stampede of the ladies, who fled, covering their heads with their hands, fearing that the dreaded little vampires might make a lodgement in their hair.

This remarkable exhibition much increased our disappointment in regard to the habitable condition of our acquisition, and was entirely unexpected, inasmuch as the unwelcome neighbors were in their dormant state and ensconced out of sight when the property was examined previous to purchase. With their appearance, and in such immense numbers, the prospect of immediate in-door arrangement and comfort vanished; the paramount the urgent necessity was to get rid of such a nuisance as quickly as possible, and the question was by what means could this be accomplished. Our scientific friends and acquaintances both in New York and Philadelphia were consulted, various volumes of natural history were examined, in order to ascertain the peculiar habits of the vermin, but we derived no effectual consolation from these sources. One of our friends, indeed, sent us from New York an infallible exterminator in the form of a recipe obtained at no inconsiderable cost: strips of fat pork saturated with a subtle poison were to be hung up in places where the annoying "creatures did most congregate"—of this they would surely eat and thus "shuffle off their mortal coil." How many revolving bat seasons it might have required by this process to kill off the multitude, the urgency of the case would not allow us to calculate, and the experiment was therefore abandoned.

Evening after evening did we patiently though not complacently watch this periodical exodus of dusky wings into light from their lurking-places one after

another, and in some instances in couples and even trebles, according as the size of the holes or apertures from which they emerged in the slate roofing would permit. Their excursions invariably commenced with the cry of the "whippoorwill" both at coming evening and at early dawn, and it was observed that they always first directed their flight towards the river, undoubtedly to damp their mouse-like snouts, but not their spirits, for it was likewise observed that they returned to play hide-and-seek and indulge in all other imaginable gambols; when, after gratifying their love of sport and satisfying their voracious appetites (as the absence of mosquitoes and gnats testified) they would re-enter their habitation, again to emerge at the first signal of their feathered trumpeter. I thus ascertained one very important fact, namely, that the bat, or the species which annoyed us, ate and drank twice in twenty-four hours. Such appeared their habit—such, therefore, was their indispensable need. Upon ascertaining this fact, after having tried suffocation by the fumes of brimstone with only partial success, I concluded to adopt a more efficient plan of warfare, and for this purpose commenced by causing all the holes, fissures in the wood-work, and apertures in the slating to be hermetically sealed with cement. This put a stop to their egress, but to avoid their dying by starvation and deprivation of water, which would much increase the annoyance by adding their dead to their living stench, I ordered apertures of about two feet square to be opened in the lathed and plastered partition on each side of the garret windows and also in the ceiling of every garret room; lastly, when the bat's reveille was sounded by the bugle of the whippoorwill, all the hands of our establishment, men and boys, each armed with a wooden implement, (shaped like a cricket-bat,) marched to the third floor "on murderous deeds with thoughts intent;" a lighted lantern was placed in the middle of one of the rooms, divested of all furniture, to allure the hidden foe from their strongholds. After closing the window to prevent all escape into the open air, the assailants distributed themselves at regular distances to avoid clubbing each other, awaited the appearance of the bats, enticed into the room by the artificial light and impelled by their own natural craving. The slaughter commenced and progressed with sanguinary vigor for several hours, or until brought to a close by the weariness of dealing the blows that made the enemy bite the dust, and overpowered by the heat and closeness of the apartment. This plan succeeded perfectly. After a few evenings of similar exercise, in which the batteurs became quite expert in the use of their weapon every wielding of the wooden bat bringing down an expiring namesake, the war terminated by the extermination of every individual of the enemy in the main building. However, there still was the cock-loft of the laundry, which gave evidence of a large population. In this case I had recourse to a plan which had been recommended, but was not carried out in regard to the dwelling-house. I employed a slater to remove a portion of the slating which required repairing. This process discovered some fifteen hundred or two thousand bats, of which the larger number were killed, and the surviving sought the barn, trees, and other places of concealment in the neighborhood.

In the main building nine thousand six hundred and forty bats, from actual counting, were destroyed. This was ascertained in the following manner: after the battling of each evening the dead were swept into one corner of the room, and in the morning, before removing them to the manure heap, they were carefully counted and recorded; many had been killed before and some few after the reckoning was made, and were not included in it, nor were those killed under the adjoining laundry roof. The massacre commenced by killing fewer the first evenings, the number increasing and then diminishing towards the end, but it was generally from fifty or a hundred, up to six hundred and fifty, the highest mortality of one evening's work, dwindling down to eight, five, three, and two.

This species of bat is generally small, black, and very lively; some smaller than the ordinary size were found, probably young ones, and one or two larger,

supposed to be grandfathers, of a reddish hue, which was thought to be from age. These vermin are generally more or less covered with a small-sized bug not very dissimilar to the common chinch, but of a different species. As previously stated, the bat has a very disagreeable odor, which also pertains to its ejection.

The manure as well as the bodies of the slain was used to fertilize the flowering and vegetable garden, and thus, in some degree, they served to compensate us for the annoyance to which we had been subjected. The manure, however, required to be applied with caution, since, if used in too large a quantity, it appeared to burn the organism of the plants.

To remove the very disagreeable odor which remained in the upper part of the house, various kinds of disinfectants were employed with some advantage; but the most effectual method resorted to was that of opening holes of about four inches square, two at each gable end, to permit a current of air to pass through. These holes were covered with iron gauze to prevent the re-entrance of any of the remainder of the army of the enemy which might hover around the premises. At the end of five years the odor has now nearly disappeared, being barely perceptible during a continuance of very damp weather.

[The fact mentioned above of the numerous parasites infesting bats is perhaps the most revolting feature in these creatures. The enormous population of *Acari* found upon their bodies is due to the great generation of animal heat in their close haunts, a condition conducive to a rapid increase of all kinds of vermin. In this country the common bed-bug (*Cimex lectularis*) is frequently found upon their fur. The entrance of a bat, with its precious burden, into the open window of a farm-house is the solution of that frequently propounded question of the despairing housewife, "Where *can* the bugs come from?"]

TABLES OF WEIGHTS AND MEASURES.

ENGLISH WEIGHTS AND MEASURES

AVOIRDUPOIS.

	Grains.	Drachms.	Ounces.	Lbs.	Qrs.	Cwt.	Tons.
Grain	1.	....	....	....	......	......	......
Drachm	27.34	1.	....	....	......	......	......
Ounce	437.5	16.	1.	....	......	......	......
Pound	7000.	256.	16.	1.	......	......	......
Quarter	196000.	7168.	448.	28.	1	......	......
Cwt	784000.	28672.	1792.	112.	4	1	......
Ton	15680000.	573440.	35840.	2240.	80	20	1

TROY WEIGHT.

	Grains.	Dwts.	Ounces.	Lb.
Grain	1	....	....	......
Pennyweight	24	1	....	......
Ounce	480	20	1	......
Pound	5760	240	12	1

1 cubic inch of distilled water, in air, at 62° F............... =252.456 gr.
1 cubic inch of distilled water, *in vacuo*, at 62° F............. =252.722 gr.

		Cubic inches.
1 gallon	=	277.276.
1 pint	=	34.659.
1 fluid ounce	=	1.7329.
1 litre	=	61.024.
1 cubic centimetre	=	0.061024.
1 cubic inch	=	16.387 cubic centimetres.

1.00000 parts of gas at 32 F., 29.922 bar., (also at 32°,) become, at 60° F., bar. 30 inches, (also at 60°) =1.05720 parts.

FRANCE.

METRICAL SYSTEM NOW IN USE.

LENGTH.

	English value.
Millimetre, (1,000th of a metre)	0.03937 inches.
Centimetre, (100th of a metre)	0.39371 inches.
Decimetre, (10th of a metre)	3.93708 inches.
Metre,* (unit of length)	39.3708 inches, or 3.2809 feet.
Decametre, (10 metres)	32.809 feet, or 10.9363 yards.
Hectometre, (100 metres)	328.09 feet, or 109.3633 yards.
Kilometre, (1,000 metres)	1093.63 yards, or 0.62138 miles.
Myriametre, (10,000 metres)	10936.33 yards, or 6.21382 miles.

* The metre is a ten-millionth part of the quadrant of the meridian of the earth, or, in other words, the ten-millionth part of the distance from the equator to the pole.

SURFACE.

	English value.
Centiare, (100th of an are, or a square metre)	1.1960 square yards.
Are, (square decametre and unit of surface)	119.6033 square yds., or 0.0247 acres.
Decare, (10 ares)	1196.033 square yds., or 0.2474 acres.
Hectare, (100 ares)	11960.33 square yds, or 2.4736 acres.

CAPACITY.

Millilitre, (1,000th of a litre, or cubic centimetre)	0.06103 cubic inches.
Centilitre, (100th of a litre)	0.61027 cubic inches.
Decilitre, (10th of a litre)	6.10270 cubic inches.
Litre, (cubic decimetre and unit of capacity)	610.2705 cubic inches, or 2.2010 galls.
Decalitre, (10 litres)	61.02705 cubic inches, or 1.7608 pts.
Hectolitre, (100 litres)	3.53166 cubic feet, or 22.0097 galls.
Kilolitre, (1,000 litres, or cubic metre.)	35.31658 cubic feet, or 220.0967 galls.
Myrialitre, (10,000 litres)	353.1658 cubic feet, or 2200.9667 galls.

SOLID.

Decistere, (10th of a stere)	3.5317 cubic feet.
Stere, (cubic metre)	35.3166 cubic feet.
Decastere, (10 steres)	353.1658 cubic feet.

WEIGHT.

Milligramme, (1,000th of a gramme)	0.0154 grains.
Centigramme, (100th of a gramme)	0.1544 grains.
Décigramme, (10th of a gramme)	1.5440 grains
Gramme, (unit of weight)	15.44 grains.
Decagramme, (10 grammes)	154.4 grains.
Hectogramme, (100 grammes)	1544 grains, 3.2167 oz. troy, or 3.5291 oz. avoirdupois.
Kilogramme, (1,000 grammes)	$32\frac{1}{6}$ oz. troy, or 2.2057 lbs. avoirdupois.
Myriagramme, (10,000 grammes)	$321\frac{2}{3}$ oz. troy, or 22.057 lbs. avoirdupois.

Value of millimetres in English inches.

Millimetres.	English inches.	Millimetres.	English inches.	Millimetres.	English inches.
1	0.03937079	45	1.7716	125	4.941
2	0.07874158	50	1.968	130	5.118
3	0.11811237	55	2.165	135	5.315
4	0.15748316	60	2.362	140	5.512
5	0.19685395	65	2.559	145	5.708
6	0.23622474	70	2.756	150	5.906
7	0.27559553	75	2.953	155	6.103
8	0.31496632	80	3.149	160	6.299
9	0.35433711	85	3.346	165	6.496
10	0.39370790	90	3.543	170	6.693
15	0.5905	95	3.740	175	6.890
20	0.7874	100	3.937	180	7.087
25	0.9842	105	4.134	185	7.284
30	1.1811	110	4.331	190	7.480
35	1.3779	115	4.528	195	7.677
40	1.5748	120	4.741	200	7.874

Table for the conversion of degrees of Centigrade thermometers into those of Fahrenheit's scale.

Cent.	Fah.	Cent.	Fah.	Cent.	Fah.	Cent.	Fah.
—100	—148.0	—49	—56.2	2	35.6	53	127.4
— 99	—146.2	—48	—54.4	3	37.4	54	129.2
— 98	—144.4	—47	—52.6	4	39.2	55	131.0
— 97	—142.6	—46	—50.8	5	41.0	56	132.8
— 96	—140.8	—45	—49.0	6	42.8	57	134.6
— 95	—139.0	—44	—47.2	7	44.6	58	136.4
— 94	—137.2	—43	—45.4	8	46.4	59	138.2
— 93	—135.4	—42	—43.6	9	48.2	60	140.0
— 92	—133.6	—41	—41.8	10	50.0	61	141.8
— 91	—131.8	—40	—40.0	11	51.8	62	143.6
— 90	—130.0	—39	—38.2	12	53.6	63	145.4
— 89	—128.2	—38	—36.4	13	55.4	64	147.2
— 88	—126.4	—37	—34.6	14	57.2	65	149.0
— 87	—124.6	—36	—32.8	15	59.0	66	150.8
— 86	—122.8	—35	—31.0	16	60.8	67	152.6
— 85	—121.0	—34	—29.2	17	62.6	68	154.4
— 84	—119.2	—33	—27.4	18	64.4	69	156.2
— 83	—117.4	—32	—25.6	19	66.2	70	158.0
— 82	—115.6	—31	—23.8	20	68.0	71	159.8
— 81	—113.8	—30	—22.0	21	69.8	72	161.6
— 80	—112.0	—29	—20.2	22	71.6	73	163.4
— 79	—110.2	—28	—18.4	23	73.4	74	165.2
— 78	—108.4	—27	—16.6	24	75.2	75	167.0
— 77	—106.6	—26	—14.8	25	77.0	76	168.8
— 76	—104.8	—25	—13.0	26	78.8	77	170.6
— 75	—103.0	—24	—11.2	27	80.6	78	172.4
— 74	—101.2	—23	— 9.4	28	82.4	79	174.2
— 73	— 99.4	—22	— 7.6	29	84.2	80	176.0
— 72	— 97.6	—21	— 5.8	30	86.0	81	177.8
— 71	— 95.8	—20	— 4.0	31	87.8	82	179.6
— 70	— 94.0	—19	— 2.2	32	89.6	83	181.4
— 69	— 92.2	—18	— 0.4	33	91.4	84	183.2
— 68	— 90.4	—17	+ 1.4	34	93.2	85	185.0
— 67	— 88.6	—16	3.2	35	95.0	86	186.8
— 66	— 86.8	—15	5.0	36	96.8	87	188.6
— 65	— 85.0	—14	6.8	37	98.6	88	190.4
— 64	— 83.2	—13	8.6	38	100.4	89	192.2
— 63	— 81.4	—12	10.4	39	102.2	90	194.0
— 62	— 79.6	—11	12.2	40	104.0	91	195.8
— 61	— 77.8	—10	14.0	41	105.8	92	197.6
— 60	— 76.0	— 9	15.8	42	107.6	93	199.4
— 59	— 74.2	— 8	17.6	43	109.4	94	201.2
— 58	— 72.4	— 7	19.4	44	111.2	95	203.0
— 57	— 70.6	— 6	21.2	45	113.0	96	204.8
— 56	— 68.8	— 5	23.0	46	114.8	97	206.6
— 55	— 67.0	— 4	24.8	47	116.6	98	208.4
— 54	— 65.2	— 3	26.6	48	118.4	99	210.2
— 53	— 63.4	— 2	28.4	49	120.2	100	212.0
— 52	— 61.6	— 1	30.2	50	122.0	101	213.8
— 51	— 59.8	— 0	32.0	51	123.8	102	215.6
— 50	— 58.0	+ 1	33.8	52	125.6	103	217.4

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